Transactions of the Twelfth South African Medical Congress.

CAPE TOWN, 1910.

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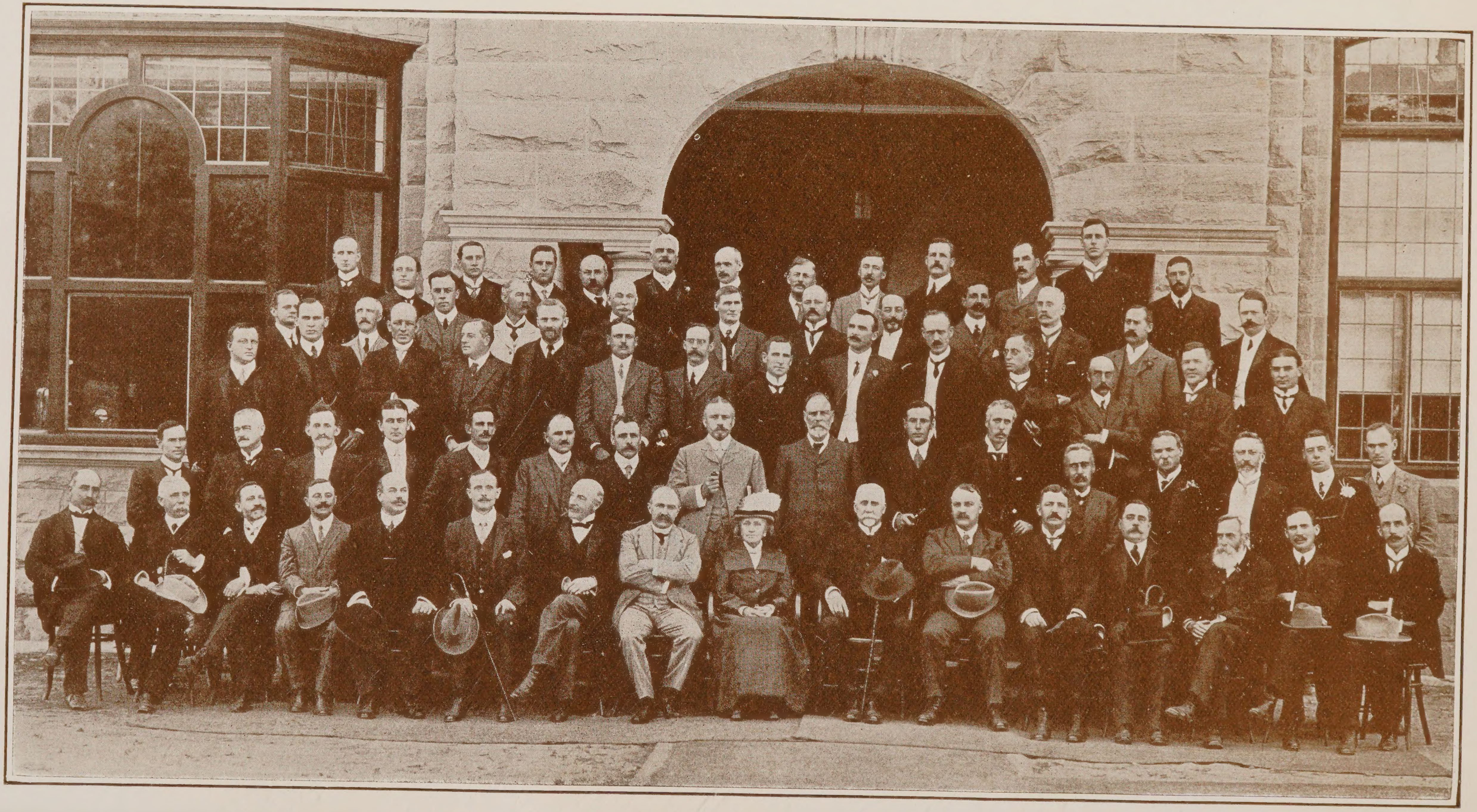

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OF THE

South African Medical Congress

(TWELFTH MEETING).

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1911.

ROYAL SOCIETY OF MEDICINE
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MEMBERS OF SOUTH AFRICAN MEDICAL CONGRESS, 1910.

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EDITORIAL NOTE.

The Transactions Committee regret that limitations of space have necessitated some curtailment of most of the Papers and the omission of bibliographies and other *addenda*. The Committee desire to acknowledge the kindness of Dr. Darley-Hartley, Editor of the *S. A. Medical Record*, in placing at their disposal his notes on several of the Discussions and the plate of the group photograph of members of the Congress.

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TWELFTH SOUTH AFRICAN MEDICAL CONGRESS (1910)

SECTION OF GENERAL SECRETARY.

ADDRESS TO H.R.H. THE DUKE OF CONNAUGHT.

Opening Ceremony in the City Hall, Cape Town,
November 2nd, 1910.

To Field-Marshal His Royal Highness Prince Arthur William Patrick Albert, Duke of Connaught and Strathearn, and Earl of Sussex, Prince of the United Kingdom of Great Britain and Ireland, K.G., Kt., K.P., Great Master and Principal Knight of the Grand Cross of the Order of the Bath, G.C.S.I., G.C.M.G., G.C.I.E., G.C.V.O., etc.:

May it please Your Royal Highness:

The Members of this Medical Congress here assembled, representing all parts of British South Africa, avail themselves of this opportunity to welcome Your Royal Highness and their Royal Highnesses the Duchess of Connaught and Strathearn and the Princess Patricia of Connaught and Strathearn, and desire to convey to Your Royal Highnesses their earnest appreciation of the honour conferred by the presence of Your Royal Highnesses to-day.

We are not unmindful of the fact that the tradition of Your Royal House has always been that of interest in and encouragement of the advancement of the Science of Medicine. We call to mind its gracious and far-reaching influence in Great Britain and the Dominions beyond the Seas; it is now our privilege in United South Africa to have an actual demonstration of that interest in the presence of Your Royal Highness.

This assemblage of medical men denotes the visible unity of the Profession, now no longer living under separate systems of Laws and Government as in the past, but under fresh auspices coming together as one Body united in one purpose. We claim that a united profession of Medicine is capable of doing much in a national sense for the welfare of the people, and we take the gracious presence of Your Royal Highness as the very best recognition of the high place in the national life that our Profession should and does fill.

Whatever may be the future in store for our Profession in this land, the fact that the Royal House of England by the presence of Your Royal Highness has marked with approval the new era of Medical Union in South Africa will always remain a treasured memory.

May it please Your Royal Highness:

This the most humble, loyal and dutiful address of the President and Members of the Twelfth South African Medical Congress.

REPLY OF H.R.H. THE DUKE OF CONNAUGHT.

It would be difficult to find, through the length and breadth of the world, a climate healthier than that which South Africa enjoys. With the dry atmosphere, abundant sunshine, and for the most part no intolerable extremes of temperature, she lacks what in European and Eastern countries are the most fruitful causes of sickness. And yet by a strange anomaly few parts of the world are exposed to the visitation of diseases more obscure in pathology or more baffling to cure.

The time is long past when a man could take all learning for his province. Even that part of knowledge which is embraced under the department of medical science is too wide for any single individual to grasp entire. In medicine, as in other sciences of like extent and importance, the labours of the general practitioner must be supported and enlightened by the researches of the specialist. And here, as elsewhere, specialisation, properly regarded, implies co-ordination.

In the single-hearted devotion of her servants to the general good, science is free from distractions of local interests and local feeling, necessary and healthy though they may be, by which political advance is complicated. And so it is that this, the first year of political Union in South Africa, is the twelfth anniversary of the meeting of the South African Medical Congress "to discuss and advance among its members, representative of all medical practitioners in South Africa, scientific and other questions of interest to the profession."

I trust that in medicine, as elsewhere, among the benefits of Union will be a more perfected co-ordination of energy, and a fuller concentration of force wherever it may be most required. In speaking of the objects for which the Congress is assembled, I omitted one, of a different nature from the rest, but not the least material—the exertion of influence upon public opinion. The advancement of medical knowledge is an end of which it is difficult to over-estimate the importance to the community. The health of its population has come to be regarded as one of the first cares of a State; and those who, with little prospect, or, at least, no immediate prospect, of material reward, devote their energies to the arduous and often costly labour of research, contributing thereby to the prevention, palliation, or eradication of disease, deserve something more than the thanks of their fellow-citizens.

Not in their own interests, but in the interests of the objects for which they labour, they may fairly claim that the path to knowledge, by its nature no less difficult than is, according to the ancient poet, the path to virtue, should at least, to some extent, be cleared of financial obstacles. I heartily sympathise with the aims and aspirations of the Congress, and wish it every success in its deliberations.

PRESIDENTIAL ADDRESS.

By Dr. Charles F. K. Murray,
President of the Congress.

Ladies and Gentlemen, and Members of this Congress, Mr. President and Members of the respective Medical Councils of the various Provinces of South Africa, and Gentlemen of the various public Bodies, who are assembled here present, I have the honour to give you cordial greeting.

To the Members of this Congress I have the pleasant duty of returning my earnest thanks for the honour they have conferred upon me in electing me to the high office which I hold. It was with considerable trepidation that I accepted this office, bearing in mind as I did the duties and responsibilities it involved.

Now it is one of the duties of your President to deliver, at the opening of your Congress, an address, and here at once a difficulty presents itself as to the nature of the material he should handle.

If he select a purely professional technical subject, it may not appeal to a large part of his audience, and that part to which it does appeal may, like Oliver Twist, ask him for more, and if he has not more to give, his position is an awkward and disappointing one.

Under these circumstances it appeared to me that the few remarks I had to offer should be of a general nature, and should aim to indicate broadly the influence of the Profession on the National Welfare, and the influence of the people upon the Profession, for whatever character this influence may take will be reciprocal, and may become either an uplifting or a degrading force.

I have frequently been asked, "What is the Medical Congress?" Therefore, I may be allowed at this stage to draw the attention of my audience to the meaning and scope of the Congress. In doing so I will ask the forbearance of my colleagues for travelling over ground with which they are already familiar.

In the first place, a reference to what might be termed our Agenda will show that the work of the Congress is mainly scientific. Each of these Sections represents special lines of work. Taken as a whole, these show the very wide range of the actual problems being dealt with day by day in South Africa.

Thus it may be readily seen how valuable may be the results of a Congress like this, which brings together the men who are working in various branches of the profession all over South Africa. It gives opportunities for discussion and comparison—for winnowing the chaff from the wheat—for measuring progress. It means encouragement to the earnest worker, balance to the enthusiast, and a co-operation which must bring strength to the profession as a whole.

Besides the purely scientific work, there are other matters within the scope of the Congress which claim our attention. These are the ethics of the profession; the attitude of its members towards each other and towards the public; its responsibility towards the moral, intellectual and physical well-being of the nation, and its reverence for that freedom of thought which is the keeping, as it were, of "The Open Door" to the ever-widening Beyond of Light and Truth.

It labours to brush away the cobwebs and accretions of the past, ever pressing forward to the Light, ever subjecting its theories and conclusions to the severest tests, and destroying without remorse many a stately dwelling-place in which it may have sojourned as soon as it may find its foundations unsound. It presses steadily onwards—its goal ever the Truth. This, Ladies and Gentlemen, is the aim of the present Congress, this the scope of its work.

The aim and object of this Congress being what I have indicated, it will be well to take note of our recent past in this Country, our Present and our Future.

Some forty years ago in Cape Colony there were only sixty registered practitioners, a mere handful comparatively to the vast area.

Indeed, the area to be covered was so vast and the men were so few that the Government were glad to avail themselves in outlying districts of the services of educated but unqualified men to act as practitioners. In some instances such men were allowed to hold the appointment of District Surgeon, and when the Medical and Pharmacy Act of 1891 came into force the position of these men had to be taken into account, and matters were adjusted by the Government duly licensing some half-dozen or more, and entering their names as licensed medical practitioners.

To the state of affairs in those early days is applicable the line of Virgil, *Apparent rari nantes in gurgite vasto.*

Although the population was much less at that time, yet the area to be traversed was the same as to-day, except that then locomotion was exceedingly difficult compared with what it is now. It was nothing unusual for a country practitioner to start from his home in the morning and not return for a week, having gone on from one farm to another. The method of work was alike hard for the patient and for the doctor.

And what of the midnight drives and rides to urgent cases over bad roads and across flooded rivers?

All honour to these medical practitioners in the past who have faithfully done their duty! Quite sure am I that many acts of self-denial and hard work little short of heroism have been done by our brothers in those outlying districts which have never been blazoned forth in the columns of the daily Press. But such times have to a great extent passed away; with the advent of roads, bridges, trains and motors, a new order of men and affairs has arisen.

The position as regards numbers is reversed, the professional field being as crowded now as it was empty then; but although that makes the conditions of professional life in some respects harder, yet on the whole it has promoted a healthy competition which prevents the individual members from crystallizing in thought or getting rusty in technique.

This is well for the profession and the public, but only within certain limits.

It would be an invidious task for me to say what branch of our work lags behind and what is on a par with that done in the older countries, but after a recent visit to Europe I returned here with the feeling that in most things we hold our own. I am quite sure that what is known as the "general practitioner" in this country is, taking the average man, doing better work and accepting larger responsibilities than the average general practitioner in Europe. So great is the craze in Europe for specialism that when any difficulty arises the patient hurries off or is taken to some specialist. Thus to the ordinary general practitioner nothing is left but the husks of practice, producing a disheartening effect, developing in the man himself a want of confidence, a want of skill in technique, and eventually turning him into a kind of medical broker, compelling him to turn over most of his interesting cases rather than accept responsibility.

Yet his is a wonderful life, with its long hours, great anxieties, and often personal risks—his highest, and sometimes his sole, reward the knowledge of work well done.

Enormous strides have been made of late, and our field of knowledge is ever extending, so much so that it is quite impossible that any one man could satisfactorily cover it all; indeed, advancing knowledge shows that it is not desirable, even if it were possible, that every kind of work should be undertaken by the same individual. This being so, specialism becomes a necessity. All this the profession has long ago recognised, and set itself to regulate and to put on a broad, scientific and honest basis.

Specialism is a source of strength to the profession, a fair division of labour, tending to advance science as a whole.

Both the general practitioner and the specialist have their place of equal honour and value in the profession as well as to the public, and it is of the highest importance that the public should understand the relations in which they stand to one another.

And what of our future? In a scientific sense we are ever searching for more light. A noble band of workers is to be found throughout the world, occupied in research, constantly subjecting not only its own work, but that of others, to the severest tests.

It is of surpassing interest to contemplate how interwoven is the science of medicine with every department of human well-being. This our knowledge of to-day more and more clearly points out.

As we advance in what is called civilization, and our lives and social conditions become more and more artificial, there is more urgent call for the study and application of scientific methods. We may sigh for the Simple Life, for more natural conditions, but we are swept along by forces we cannot resist, and our only course is to arm ourselves to control these forces.

The wealth of a nation is the health of its people, and good health tends to develop good character. That this is being recognised is shown by the ever-growing tendency to legislation in social matters.

When once we begin to interfere with the methods of Nature, we have to extend our responsibility proportionately. For instance, our philanthropic fostering of the "unfit" is forcing upon us the gravest problems, as may be seen from a perusal of the Report of the Poor Law Commission.

We, in a young country, have the advantage of a comparatively "clean slate" as well as of the lessons from the experience of older countries, but if we neglect the rapidly passing opportunity, we shall soon be overwhelmed with an accumulation of evil beyond our power to deal with.

We all know how these problems of the "Unemployable," the "Unfit," the "Criminal," and the "Feeble-minded" are pressing in the older countries. Do not let us neglect them here while they are still comparatively manageable.

Herbert Spencer tells us in the "Study of Sociology":—"Fostering the good-for-nothing at the expense of the good is an extreme cruelty —it is a deliberate storing up of miseries for future generations. There is no greater curse to posterity than that of bequeathing them an increasing population of Imbeciles, Idlers and Criminals."

This was written years ago, but let us turn to what is written on this subject to-day in the *Eugenics Review* for April by Mr. Newton Crane, upon " Marriage Laws, and Statutory Experiments in Eugenics in the United States." A perusal of this article will give rise to much thought, but the gist of it is that marriage should come under the strictest scrutiny from the standpoint of Public Health. The marriage of the feeble-minded, imbecile, insane, drunkard, epileptic is now forbidden in many of the States. In Indiana certain very drastic provisions are laid down for dealing with such persons. In Connecticut certain special conditions are laid down, calling upon the State Board to take action to prevent any possibility of an increase of the feeble-minded, imbecile, or insane.

Time prevents me from doing more than merely touching upon this point, but surely it is my duty to ask all thoughtful public men who love this country not to allow themselves to be submerged in party politics, but to rise above them in order to grapple with a great and increasing danger.

To do this it will be absolutely necessary to create machinery which can only be done through a Portfolio of Public Health, whose duties will be to collect evidence, to seek out the causes of evils, and to educate the public conscience as to responsibility towards the future of the race.

It seems to me, and to the profession I have the honour to represent, incredible that any statesman who has the welfare of his country at heart could think that it was possible to do full justice to its growing needs unless such machinery be evolved.

It seems hardly credible that to-day in this country more is spent by the State in regard to the study of the diseases of animals than in those of the human race.

Should we not, at this time of Union, set ourselves to readjust this state of things, and take at least the fundamental step so unanimously urged by the profession—the creation of a Portfolio of Public Health?

Sure am I, unless this step be taken, South Africa can make no advance in real prosperity. Delay in this matter can only mean accumulating evil, which must eventually add enormously both to the difficulty and cost.

BUSINESS MEETING OF CONGRESS, NOVEMBER 4TH, 1910.

RESOLUTIONS ADOPTED UNANIMOUSLY.

A. Resolutions submitted by a Conference of Delegates from all the Medical Societies in South Africa:—

I. That a Portfolio of Public Health be created, to be held if need be in combination with some other Portfolio.

II. That a Medical Officer of Health for South Africa be appointed, who shall be directly responsible to the Minister and be the sole administrative head of his department. That he be a qualified expert in his department; and that his actions shall be confined to Public Health matters.

III. That a Medical Council for the Union shall be created, which shall be chiefly elective, and shall have control of admission to the practice of medicine, of dentistry, of midwifery, and of nursing, and shall have control of the discipline of these professions.

IV. That upon any change of policy on medical questions other than those of Public Health, or upon any question affecting the status of these professions, the Medical Council shall be consulted.

V. That there shall be an adequate representation of medical men on all Boards and Committees which have to do with the establishment, maintenance and management of hospitals under the Provincial Councils.

B. Resolution submitted by the Section of Medicine:—

That this Congress, recognising the serious prevalence of Tuberculosis among the European and coloured races of this country, and the inadequacy of the present means of coping with the disease, respectfully requests the Government to appoint a Commission to investigate the amount of Tuberculosis among the white and coloured races of the Union, what steps are being taken in the different Provinces to deal with the disease, and what provisions exist for research and for the treatment of Tuberculosis according to the best methods known to modern science.

C. Resolutions submitted by the Section of Public Health:—

I. That the attention of the Government be called to the great desirability of the early institution throughout the Union of a uniform nomenclature and system of Deaths Registration, and that in the drawing up of such a nomenclature one of the matters which should receive special consideration is the classification and grouping of the several forms of diarrhœal diseases.

II. That Congress recommends that when new legislation regarding the sale of milk is introduced, (1) Power be given to Local Authorities—(*a*) to adopt a bacteriological standard for milk offered for sale, and (*b*) to refuse the issue of dairy licences to persons who are unsuitable on hygienic grounds. (2) That machinery be introduced for the efficient inspection and control of all places where milk is produced for sale.

III. That Congress is of opinion that the prevalence of Syphilis amongst Natives throughout the Union demands immediate attention, and that it should be one of the first duties of the Medical Officer of Health for the Union, upon the organisation of a Union Health Department, to deal with it. That Congress is convinced that this disease is not only a great and increasing danger to both Europeans and Natives, but is operating as a powerful agent in the physical degeneration of the latter.

D. Resolutions submitted by the Section of Special Subjects:—

I. That whereas the Medical Profession is now, as ever, prepared to give free assistance to those unable to pay for the same, this Congress affirms the advisability of the establishment of hospitals entirely or partly supported by Government for such cases only. In centres large enough to warrant such establishments, these establishments should be entirely apart from any hospital, or department of the same, receiving patients paying either in part or in whole.

II. That where satisfactory nursing homes are available to any registered practitioner, patients able to pay for treatment in such homes shall not be admitted into State-aided hospitals.

III. That to meet the necessities of patients in large centres only able to pay part fees for hospital accommodation, either intermediate hospitals be established, or separate departments of hospitals set aside for such purpose.

IV. That where a nursing home is not available patients be admitted into State-aided hospitals at fees from 3s. 6d. upwards, and shall be considered private patients, able to select their own medical practitioner and be responsible for their fees to such medical practitioner.

V. That in the classification of patients, the most satisfactory form of evidence is a certificate from a medical practitioner.

E. Unopposed resolutions:—

I. That this meeting of the South African Medical Congress draw the attention of the Union Government to the passage in H.R.H.'s address at the Opening Ceremony referring to the necessity for financial support for medical research in South Africa, and urge the Government to make provision to enable research workers to prosecute their work unhampered by financial considerations; that this Congress also make an appeal to the wealthy in South Africa and elsewhere for aid and support in furthering medical research.

II. That this Congress protests most strongly against the transference of Public Health, or any other matters affecting the Medical Profession, from the Union Government to the Provincial Councils.

III. That Congress draw the attention of the Union Railway Management to the unsatisfactory supply of drinking water on trains.

IV. That these Resolutions of Congress be referred to the Standing Committee, with instructions to take steps to urge them before the proper authorities.

A.—SECTION OF MEDICINE.

I.—THE MODERN TREATMENT OF TUBERCULOSIS.

(Subject for Special Discussion.)

By A. Murray Gray, L.R.C.P., Etc., Durban.

Tuberculosis is the most common of all diseases, and it exists in the pulmonary form more frequently than in any other; it is therefore my intention to consider its modern treatment, principally as it affects the lungs.

The open-air cure of tuberculosis is not so very new after all, and I need but remind you that an English physician, George Bodington, of Sutton Coldfield, in 1840, wrote a book advocating the treatment of the disease on principles which are, to all intents and purposes, the same that we now employ, and by putting those principles into practice he obtained many cures.

Bremner well states these principles when he says that they consist of:—(*a*) A life spent in the open air under conditions which give iimmunity from tuberculosis; (*b*) complete freedom from any debilitating circumstances, or anything which may lead to an exacerbation of the disease; (*c*) methodical hill-climbing as an exercise when the condition of the patient renders this advisable; (*d*) constant and unremitting medical supervision; (*e*) an abundant dietary, in which milk, fatty food, and vegetables occupy an important place; (*f*) various hydro-therapeutic measures.

Probably twenty-five years ago the average man would sum this up by saying that it was necessary to keep the patient in the open-air, no matter what the wind or the temperature; that he should be forced with food, no matter what the result in the production of useless fat; that he be exposed to the sun, no matter what the severity of its rays; and generally be brought to the condition of a heavy, short-winded, more or less useless individual, reclining in the open during the day, and at night possibly sleeping with a cold, cutting wind playing round his bed, or even the snow falling on his coverlet. That some cures and many "arrested" cases were obtained by this crude treatment was a fact; but we have progressed since then, and can point to a more successful, a more scientific, method which Bremner's statement sums up.

The modern treatment of tuberculosis is the result of careful observations made in efficient sanatoria, of increasing knowledge of how to apply rest and exercise as curative agencies, of immunity, and of what the use of tuberculin can and cannot effect.

Formerly the question of climate was regarded as paramount; now we can say with every assurance, viewing the successful results of treatment in a great variety of climates, that no particular climate or altitude is a specific, though we still know that these matters are important factors in treatment. As Trudeau says: " It seems hardly rational to insist upon the minutest details of the patient's surroundings and habits of life and deny absolutely any influence of climate as a factor in securing the most favourable environment obtainable for the patient." Tuberculosis undoubtedly exists less in dry regions, in dry forest places, or on elevated desert lands. At the same time any climate will do for its treatment provided the air is pure and bracing; and one would naturally choose a place where the rainfall is reasonably small, where there is a maximum of sunshine, where there is a sandy subsoil to promote surface drainage, and where protection from high winds can be secured. Strong, cold winds are undoubtedly injurious, the bad effects being brought about by the dyspnœa they cause and by the great loss of heat which takes place owing to the quantity of air which passes over the body. If the patient be too much exposed to the influence of strong sunshine, too, it will have a debilitating effect, and some authorities hold he will be rendered more liable to hæmoptysis.

All active tuberculosis with fever demands rest. If there is a mouth temperature of 99° F. in men or 99.2° F. in women they should have rest; if still higher, they should rest in bed; and if the temperature rises above 100.5° F., should have absolute rest till it comes down.

The proper regulation of rest and exercise is one of the most difficult things to decide upon in these cases. Over-exertion in any form is pernicious, and may be easily fatal, and all physical and mental excitement must be avoided; indeed, as Bremner says, everything which may lead to an exacerbation eliminated, or the damaged tissue has not time to heal. Wright has taught us that, in early stages, slight exertion causes auto-inoculation, whereas, when healing is more advanced, considerable exertion may be undertaken without any, or but very little.

The best way to increase the cardiac power and to promote the general health is the prescribing of such exercise as the patient's condition renders possible. I know of no absolute definite rules regarding this, but we have learnt a great deal from sanatorium treatment and know that in the majority of quiescent cases the best form of regular exercise is uphill climbing. This must be gradual and deliberate; the patient should start uphill and return downhill; the gradient should be easy and the pace quiet, not as a rule exceeding from one and a half to two miles an hour. We must not permit hurry, because any increase in the number of respirations will do harm by increasing the work of the lungs. If the patient returns with an elevated temperature, with increased cough, and perhaps headache, too much has been done; rest must be enjoyed and at the next essay less attempted. If, on the other hand, there are no bad symptoms, we may gradually permit more, till a good many miles a day are covered.

Paterson, at the Frimley Sanatorium, has gone further than this, and with most wonderful results. He selects his patients, and chooses those preferably who have already had some months' treatment in a hospital or sanatorium. He prescribes graduated labour and has evolved a very complete plan of this. And further than this, he does not discharge any case as " arrested " till the patient is able to perform, without the usual rests, tasks which are heavier than he is likely to be called upon to do on returning to his work, and has succeeded in doing these tasks for three weeks and maintaining during that time good health (*vide* Appendix). Inman has shown that this labour produces auto-inoculation, and Wright has shown that by massaging a tuberculosis lesion or moving a tuberculosis joint the opsonic index is altered, no doubt due to the movement sending an increased amount of the patient's self-manufactured tuberculin into the circulation. It appears to me that the great advantage of Paterson's method of auto-inoculation with tuberculin, brought about by graduated exercise, is the supply of "homologous" toxin in place of the foreign strain introduced with tuberculin. Paterson himself thus summarises: "Those institutions which have definitely adopted a system of graduated labour, laid down on lines of auto-inoculation, and have therefore paid heed to the importance of a mouth temperature of 99° F., plus a headache, and the necessity, should this combination occur, of putting the patient upon absolute rest, have found the system practical, and one which not only helps to arrest pulmonary disease and restores the general condition of the patient, but, further than this, enables patients to leave the sanatorium with the knowledge that they are capable of hard work."

The question of feeding is a most important one and oftentimes difficult; and it must be borne in mind that gain in weight is a very reliable guide to the patient's doing well. Each case should be weighed on the same machine and under the same conditions every week. It has been well said that an engine is not stronger than its boiler or a man than his stomach, and "that diet is best which taxes the alimentary powers least and furnishes the greatest amount of nourishment." In most early cases of tuberculosis the digestion and assimilation are good and it is merely necessary to supply good varied food, of a sufficiency not only to maintain adequate nourishment, but great enough to leave a margin upon which weight may be gained. The open-air treatment itself is, in a large number of cases, the best aid to both appetite and digestion and the patient gets not only an air-hunger but also a food-hunger. At other times we get such a condition of toxæmia, caused by the tuberculosis that the happy result does not accrue; and of course we may have to deal with a dyspeptic from the start. Often the gastric juice in these cases is deficient in hydrochloric acid and this will bring in its train fermentation and flatulency. Raw meat is here very valuable by stimulating the secretion of a highly acid gastric juice, which further stimulates pancreatic digestion on passing into the duodenum. Raw eggs, too, are of great value, particularly where sufficient ordinary food cannot be retained, and they make scarcely any demand upon the digestion if given alone. Personally, I have found the greatest benefit arise from giving one or more raw eggs, to be swallowed early in the morning a sufficient time before breakfast.

The temperature is a great help to us in prescribing food in tuberculosis; and we have to remember that in this disease often the best way of reducing temperature is by ordering solid food, sometimes with, I think, a little alcohol. When there is much toxæmia we generally get a swinging or elevated temperature.

In cases with much intestinal decomposition I have often seen good arise from following the plan of Dr. Isabella Mears. of the Woodburn Sanatorium, Edinburgh. Milk is inoculated with the *Bacillus lactis,* incubated for two hours before administration, and neutralised wih a little chalk. It is then given two-hourly, with no other food for two days, and thus the intestine is thoroughly flooded with the *Bacillus lactis.* Oftentimes the temperature comes rapidly down, and then the ordinary food can be resumed.

We do not, in modern treatment, seek to feed our patients indiscriminately, and Bardswell, Chapman and others have clearly shown that good results are not obtained by very large diets. We should aim at the patient putting on flesh rather than fat, at giving him strength and muscle by an abundant dietary to which an excess of fatty material, such as butter, is added. Tuberculous cases digest fat well. With this we combine suitable exercise and labour. Milk is most valuable, and three or more pints may generally be taken daily; but we must not forget that milk inhibits gastric secretion and so, when the digestion is bad, it had better be given between meals. It is in these cases often well to wash out the mucus from the stomach by giving hot water to sip before food. There should be a sufficient interval between the meals, and rest in the recumbent position should be taken both before and afterwards.

The hygiene of the skin should be rigidly attended to and baths are invaluable; nor must frequent washing of the teeth and mouth be omitted, and whenever needful the care of the dentist secured.

Possible re-infection must always be borne in mind and guarded against, and in this connection the finding of tubercle bacilli in the fæces, as lately demonstrated by the investigations carried on in the laboratory attached to the Royal Victoria Dispensary in Edinburgh appears to me of interest and importance.

In private practice it is not, as a rule, easy to get the rectal temperature taken, and I believe, if taken in the mouth with ordinary care, and leaving the thermometer in for five minutes, we are given all the information we need for practical clinical work. The temperature may be well taken at 8 a.m., between 4 and 6 p.m., and again at bedtime, and it is sometimes needful to have a more frequent record than this. The temperature is, of course, the great guide as to how much rest we must give, what amount of exercise or work we can allow, and as to the general progress of the case. I feel sure that in tuberculosis the thermometer to take the temperature and the watch to take the pulse are at least as important as the stethoscope with which to examine the chest. The chest should be examined every month or we cannot maintain a proper control over the case.

I believe we are safe in saying that in tuberculosis more quack remedies have been vaunted than in almost any other disease, more shameless lying has been practised, more people ruined of their chances and robbed of their money. One can only look to increasing education as to what tuberculosis really is to stem the tide of this immorality. Even in our own profession and with the purest and best intentions the number of so-called specifics tried, and for a time even lauded, by some of the great leaders, has been legion. But our modern pathology, bacteriology, and clinical knowledge have enabled us to sweep these aside, at least so far as any specific effect is intended and to turn with relief to what we know and are daily learning with regard to the usefulness of tuberculin.

Unquestionably damage was done when Koch first introduced the form of tuberculin now known as Old Tuberculin, T.O.A., as a curative agent. It was by many considered as the only thing needed by way of treatment, was given in large doses and its action was not understood; in a large number of cases there can be no doubt but that a fatal issue was hastened. Tuberculin, T.O.A., is now almost entirely used as a diagnostic agent, and that principally in animals. It is a glycerine extract from cultures of the tubercle bacillus and its action is in no way analogous to the tuberculin now in use for curative purposes. It caused reaction of the tissues round the lesions, and then the diseased material was cast off. It is thus easy to see that danger might readily ensue when dealing with such a deeply-situated organ as the lung, from whence the cast-off matter could not readily be expelled. The result of the abuse of T.O.A. was that to a great extent it was abandoned in therapeutics; and this position was, generally speaking, maintained till Koch introduced his "New Tuberculin," or "Tuberculin T.R., in 1897. This is an emulsion of pulverised tubercle bacilli, and represents the "vaccines" generally. Further stimulus was given to the use of tuberculin by the work of Wright on opsonins, etc. In the preparation of Tuberculin, T.R., living fresh virulent cultures of tubercle bacillus are dried *in vacuo* and then comminuted to an impalpable dust by machinery. The dust is then treated with saline solution and the mixture thoroughly centrifugalized. Thus an opalescent fluid, T.O. (O = *oberste;* Angl., uppermost), is obtained, together with a deposit. The deposit is worked up with successive quantities of water, and is called T.R. (R. = *rest;* Angl., residue). In 1901 Koch used a mixture of T.O. and T.R. under the name of "Koch's new Tuberculin Bacillary Emulsion."

I need not remind you that there are many other preparations of tuberculin, but the great thing is to choose a good and easily-procured one, and to thoroughly study it. As I myself have almost exclusively used Tuberculin T.R., I prefer to speak with reference to the action of this preparation. I believe the "extracts," such as the old Tuberculin T.O.A., are stated to produce only a toxic immunity, and that the "vaccines" are needed to produce an active anti-bacterial effect. But Wright says the extracts also act as vaccines, and the bacillary emulsion has the advantage of being both a vaccine and an extract. Koch's New Tuberculin T.R. is manufactured only by Meister, Lucius and Brüning, and is put up by Allen and Hanbury, in azoule form, in exact doses of any required amount, and it is in this form I use it. If there be any doubt as to its sterility, it can be heated to 70° C. for ten minutes, and it should be kept in a cold, dark place. T.R. is preserved in twenty per cent. glycerine solution, and is standardised to contain exactly ten milligrammes of solid substance in each c.c.

T.R. acts by stimulating the tissues of the body to form antibodies to the tubercle bacilli. Thus it is really immunity we are striving after, an immunity which we must bear in mind is not an immunity to tuberculosis, the aim being that, by producing tuberculin immunity, we aid the body forces by removing the toxæmia to overcome the tubercle bacillus. T.R. appears to have the power of increasing the agglutinating power of the serum as well as its opsonic properties, and there is good reason to believe that these increase concomitantly with its antibacterial properties. It is administered hypodermically, though of late very good clinicians claim to have obtained just as good results by giving it by the month, early in the morning on an empty stomach, combined with a little normal saline solution or milk. I am aware that many bacteriologists deny the good of this, but when we know that it is thus frequently used by such men as Arthur Latham, R. W. Phillip, and H. Clarke, I think it is at least worthy of trial, besides being much simpler and easier of administration in private practice, although Wright says it is like "pouring new wine into old bottles!"

The dose is measured and expressed in fractions of a milligramme of solid substance. Probably in the larger number of cases 1/2000 milligramme is quite enough to start with. Claude Rivière recommends for infants one-tenth of the adult dose, and a quarter to half a dose for older children. I am now speaking of tuberculin prepared from human tubercle bacilli, but the same preparation is made using the bovine bacillus and is then designated Perlsucht Tuberculin or P.T.R.

Tuberculin is most useful in pulmonary tuberculosis when the lesion is early and should therefore not be relegated to the position of a "last resort," though there is abundant evidence to show that even in older trouble, when the lesion is cut off by fibrous tissue, minute doses do much good; it has been said that secondary infections are not so liable to occur when it is used.

The English, or as it has been called, the scientific method, is to give small doses at intervals of one to two weeks. The German, or, as it has been called, the clinical method, is, beginning with larger doses, to give it at short intervals, increasing the dose till a febrile reaction is obtained. In this method "the guide to treatment is the patient's temperature. If this rises after any dose up to 100° F., the treatment is suspended until the temperature returns to normal. Then the last dose is repeated." One therefore depends in this method upon clinical evidence as to how to proceed, whereas in the scientific method, as laid down by Wright, frequent determinations of the opsonic index must be made, which, on account of the time, experience, and expense involved, would greatly curtail the use of tuberculin in ordinary medical practice. Wright has insisted upon the importance of not repeating a dose till the effect of the previous one has passed off; he has further taught us that immunisation can be brought about by very small doses and that it is very easy to overstep the mark.

I think but few of us in busy practice are in a position to use tuberculin controlled by the opsonic index, even supposing that this were needful; the researches of Inman have placed our clinical work on a good basis and enabled us to use tuberculin with confidence, by having shown that there is a relation between the opsonic curve and the temperature. He has demonstrated, (and his researches have been confirmed by other observers), that the opsonic index varies inversely with the temperature—when the temperature is high the index is low, and *vice versa*. We find that the injection of a moderate dose is followed by an initial rise, reaching its climax in about three hours, and then followed by a negative phase lasting three days. A positive phase follows, lasting six days, and finally the index falls to about its previous level, the whole lasting from ten to fourteen days. Thus we may take the temperature as our guide, with the assurance that by so doing we are proceding in the most rational and scientific way. Whilst no rule can be laid down, it is very important not to give a dose if there are any signs of an extension of the disease, if there is any increase of fever, or if an elevated temperature is markedly reduced by tuberculin; it is not wise to increase the dose till this effect be lost. If the general condition be good, a dose may be given about once a fortnight, and continued for three months or co. The dose may be very gradually increased, but Latham says should never exceed 1-20th milligramme. I believe tuberculin should be merely looked upon clinically as an adjuvant to other measures, for it is not a specific curative agent like mercury is for syphilis or quinine for malaria. A good motto, when using tuberculin, is, I think, *festina lente*, and no hurry should ever be used, for it is a potent remedy for evil as well as good. One must be guided by a consideration of temperature and clinical symptoms. Whilst it is oftentimes a most useful help I do not believe there is anything but danger in the teaching of Camac Wilkinson, who appears to suggest that, whatever else you give up in treating tuberculosis, hold fast to tuberculin! Tuberculin is a valuable remedy in tuberculous disease of bones and joints. Good results have been reported in tuberculous ophthalmic disease, and it is very valuable in genito-urinary and peritoneal tuberculosis, combined, of course, with general hygienic measures. Inasmuch as Weber has shown that half the tuberculous peritonitis in children depends upon the bovine bacillus, we may expect in many of these cases better results by using P.T.R.

In pulmonary tuberculosis, when tuberculin is doing good, we get an improved general condition, fewer relapses, and more patients (than without it) lose the tubercle bacilli in their sputum.

Mixed infections, which are so common in pulmonary tuberculosis, are serious things, and responsible for a large part of the mortality; it is, of course, natural that we should, following up Wright's work and teaching, here use bacterial vaccines. A good deal of work has been done, but I can only refer to the plan which is being carried out at the Sanatorium at Yelverton, in South Devon. Here, after the patient has used an antiseptic mouth-wash, the sputum is obtained and cultures grown, stained by Gram's method and the organism that is in excess of any other treated with the appropriate serum (polyvalent), and this is repeated weekly till the mixed infections disappear; the results are said to be very good. Gerald B. Webb, writing last year, says: "I would put forth the earnest plea that these potent remedies be used early, whilst the machinery of immunisation is yet unworn, and that they be added to the tuberculin treatment . . . Then the ranks of the advanced tuberculous will be lessened."

In the early stages of tubercular gland trouble tuberculin has proved its value; but when, as is so often the case, we have a mixed infection, and an infection caused too often by organisms which cannot always be cultivated, and when caseation and softening occur, I think we should not hesitate to resort to surgery.

In tubercular laryngitis, where the disease is localised or, as I think more often we shall find, there is disease in the lungs, but slight and not progressing rapidly, tuberculin has proved its value. Bier's passive congestion has been proved very useful in these cases, particularly with dysphagia, which is often a marked feature. Within the last few weeks I have obtained great relief in one of these cases, by adopting the simple expedient, suggested by Howarth, of using an elastic band, one inch wide, fitted with a small pad which lies in the notch between the cricoid and the sternum, and provided with a hook-and-eye attachment. This is easily made, and the band is worn tight enough to produce a slight flush above the region constricted. By beginning gradually, after a few days this band can be worn for twenty-two out of the twenty-four hours without any discomfort. The open-air treatment, combined with rest to the voice, is very satisfactory in its effect in these cases, and modern surgrey can do much to aid us.

The source ot cough should be ascertained in all cases and treated, but the routine giving of cough mixtures is, in modern treatment rightly condemned. We must not forget that fever in often kept up by cough, and accompanying sleeplessness results, and then some simple linctus, probably containing small doses of heroin, is invaluable. The patient can be taught to restrain useless cough.

When one remembers that about one-twelfth of the *post-mortem* examinations made in the general hospitals of London give evidence of healed tubercle, we should be encouraged to persevere, for often what look bad cases turn out well; and it is by no means always the case that those whom we get under treatment early run so good a course as even those who come under our care when more advanced. Nevertheless we should insist upon the advantages of early treatment. I feel convinced that the "individuality," so to say, of the patient, comes in, and whilst some early cases defy us, others with greater disease respond and become "arrested." Probably those who thus go on to "arrest" possess more earnestness of purpose, more common sense, courage, and patience; for it has been well said that "a fool never gets well of consumption"! The medical adviser, too, needs all these good qualities, and an ability to consider the case not only from the point of view of tuberculosis alone, but to take into consideration every circumstance connected with the patient and his environment which makes for good or evil.

Finally, and in spite of the very divergent views being at the present time expressed in letters to the *British Medical Journal,* I consider the sanatorium a most valuable part of our armamentarium. To those who doubt this I commend Bardswell's little book, published at the beginning of this year, containing as it does a most able and full analysis of the careers of 241 patients who were under his care in sanatoria during the years 1899 to 1905, and including in each the after-history on leaving the sanatorium. In his commentary on these he says: "My own figures indicate that permanent recovery may be looked for in from seventy to eighty per cent. of cases of early consumption, in from forty to sixty per cent. of cases of moderately advanced disease, and in some eight to ten per cent. only of those suffering from the disease in its more advanced stages." It is, however, right to mention that Bardswell's cases were, so far as this list goes, patients who were well-to-do or comfortably off as regards means, but they were quite unselected and were consecutive admissions to two sanatoria. I believe that if we as an Association were to stimulate and direct governments and municipalities to erect a few sanatoria in this country, and I should lay great stress upon their being both cheap in erection and efficient in practice, we should do much good, not only from the curative, but from the educational standpoint. Then, with proper health regulations, compulsory throughout the Union of South Africa, with compulsory notification and prohibition of spitting, with labour colonies, camps for curable and homes for the segregation of hopeless cases, together with municipal bureaux and dispensaries and the seeking of the disease in the homes of the people, we might look forward to a rapid diminution of what is, to my mind, at present a serious menace to our civilisation in this sub-continent.

In this connection it is very pleasant to read, in the report of the Medical Officer of Health for Cape Town, for this year, what excellent work is being done, and with what good results. I can only hope that all Dr. Jasper Anderson wants to thoroughly meet his needs in the matter of the control of tuberculosis will soon be at his disposal, and it may interest many of you to know that Durban is following on the same lines. When we learn that the cases in Cape Town are by no means severely selected, it is most encouraging to read that "in spite of this laxity of admission, I consider the results of treatment are very satisfactory and show clearly the advantages of fresh air, a good and plentiful supply, careful nursing, attention to detail, hygienic matters, and graduated labour."

APPENDIX.

Graduated Labour in Tuberculosis.

Dr. Paterson's grades are as follows:—

(1) Slow walking exercise, beginning at two miles a day and gradually increasing up to ten miles a day. (2) Picking up fir-cones and firewood in the grounds, and carrying a half-basket (weight, 11 lbs.) to the stack. (3) Carrying a full basket of firewood and cones (16 lbs.) (4) Carrying a half-basket of gravel or stones from the gravel pit to the place where paths are being made or repaired (weight, 21 lbs.) (5) Carrying a basket of gravel or stones, the weight of which is gradually increased up to 38 lbs. (6) Rolling the grass or gravel (sixteen men to pull a roller weighing 15 cwt). (7) Digging ground already broken. (8) Mowing grass with a lawn-mower. (9) Digging unbroken ground. (10) The same as under (9), but for six hours daily instead of four hours—*i.e.*, the hours usually spent at rest are spent in labour.

Plan.—A patient is first placed on grade 1, and then, as improvement shows itself, successively on grades 2, 3, etc. If the work of one grade is found to lead to rise of temperature, or other bad symptoms, the patient is at once placed on a lower grade. No patient is classified on discharge as "arrested" unless for three weeks continuously he can pass one or other of the following tests:

Test A (for a patient who earns his living by manual labour).—To be able, on an ordinary diet and without rest hours, to use a pick and shovel of the full size and weight for six hours daily and maintain his health.

Test B (for a patient who does not earn his living by manual labour).—To be able, on an ordinary diet, to perform the labour of grade No. 6 or No. 7 for three weeks, and to maintain his health. These patients are gradually brought up to No. 9, and when it is found they can do this work, they are put back to No. 6 or No. 7.

DISCUSSION.

DR. GUILLEMARD expressed his sense of the usefulness of Von Pirquet's cutaneous test, which was valuable for private practice and schools. The treatment of mixed infections was a most hopeful line and involved no serious risks, whilst he believed that deaths were mostly due to mixed infections. The idea of vaccinating the patient from his own individual strain was an excellent one. The author's particulars about convalescent sanatoria and tuberculin treatment were most interesting. Practically nothing had been done in South Africa for tuberculosis, which carried off more people than any other disease. An Association formed in Cape Town had worked under great difficulties and its outcome was very small. One had Dr. Anderson's small chalets and the work of the Dispensary under Dr. Morris, but all this was only a flea-bite. The hospitals shut their doors to the patient by extraordinary restrictions. The profession needed education on the subject. The public had been educated to the point of shunning the consumptive as a leper and blocking him everywhere from earning a livelihood. The public wanted further education. There was no single modern sanatorium existing, although even backward countries like the Argentine were provided with them. He would move that Congress, recognising the seriousness of the danger, requests the Government to appoint a Commission to go into the whole subject. It was quite impossible to carry on research in private practice, and, moreover, all modern means of treatment required public institutions.

DR. A. MACKENZIE said that the profession ought to exploit new methods of treating tuberculosis, and he did not think that so far they paid as much attention to it as the subject deserved. Its extinction was theoretically possible, but practically it was not. Not being a disease with an ascertained period of incubation, one could not take the same stamping-out measures as in many other diseases. Children showed no signs for the first twelve months or so, and the real incidence of the disease came in adolescence, consequently increasing the danger of infection. He believed in the cutaneous method supplemented by tuberculin injection. Children could harbour a disease and carry infection without showing any symptoms. If with improved methods of diagnosis we could detect the disease in them early, we might do a great deal towards stamping out infection. The influence of heredity was doubtful, but that of environment certain. If children were not put into clean surroundings, they were carriers of infection, and he believed that the public should be brought to realise that the hygiene of children was a great measure to be adopted. A large proportion of children after the first year showed latent infection. He then dwelt upon the great utility of tuberculin injections in surgical tubercular infections. Sanatorium treatment was not much better than sound methods carried on in private, except that they had the advantage of putting patients under proper discipline, but there should be supplied hospitals for advanced cases, mainly to prevent infection. Earlier cases could be treated just as well in private in small shelters with the aid of tuberculin. The eye reaction was very useful, but not very safe. He seconded Dr. Guillemard's proposition.

DR. DE VOS HUGO condemned the apathy of the Government. As regards isolation, he had built small shelters at a cost of £16 10s. each, and these could be built everywhere.

Dr. Waldron said the prevalence of the disease was appalling amongst coloured people, who were proportionately much more affected than whites. The creation of sanatoria was beyond the powers of ordinary local bodies, and ought to be done by the Government, but there seemed to be a very general idea that small shelters could be put up by local bodies. He was strongly impressed by the great number of cases due to house infection, and insanitary houses ought to be remorsely destroyed. Mud floors were especially responsible.

Dr. Bensusan referred to Dr. Ramsbottom's paper at the Bloemfontein Conference, as giving stern warning based on what had happened in the Free State, and yet nothing had been done. He agreed with the last speaker as to the danger of house infection. The bacilli never died, and the only thing to do was the pull the house down. As regards treatment, he would put serum therapy first, diet second, and sanatoria third. Graduated physical exercise was most valuable; it exercised the respiratory muscles, and the tubercle bacillus does not like being disturbed. A diet free from sodium chloride was valuable. He emphasised the dangers of the vaccine treatment. In diagnosis he had used both Von Pirquet's and Calmette's methods with good results.

Dr. D. P. Marais, as Secretary of the Cape Town Association, spoke of the hard fight they had had. They had approached the Municipal Congress, and had got that body to support them. Sanatoria on a large scale were not possible in this country, but they could isolate the bad cases in small sanatoria supported by a combination of municipalities. The bad cases infected house after house as, because of their poverty, they drifted from one to another on account of failing to pay their rent. There had been a set-back against opsonin estimation as a diagnostic guide, on account of its being proved to be replaceable by temperature study. The real treatment was not tuberculin, which was dangerous and uncertain, but graduated exercises in milder cases, and inducing pulmonary hyperæmia in those with a temperature. Von Pirquet's method was valuable in children. Calmette's more so in adults. The eye test had the advantage of being much more delicate in active mischief, Von Pirquet's more in latent mischief. The citrate test was of great value in detecting bacilli when young. He dwelt also on Phillips's method for detecting bacilli in the fæces, and the value of iodine inhalations and of the proprietary preparation called "Guaiacoles." Alluding to the value of dietetics, he emphasised strongly the usefulness of raw meat.

Dr. Tomory spoke from the public health side. Work was often done at the wrong end, and most attention had been directed to the danger of infection from native servants. He detailed the appalling condition of things in the Native Location at Bloemfontein, and pointed that the danger is increased by the fact of the native not seeking advice until too far gone. The Bloemfontein Council, however, was meditating erecting superior buildings which would be capable of ready disinfection. Flies might also be a medium of infection. He condemned the use of rubber teats and comforters as often conveying tubercular infection from native servants. In the Free State tubercular disease, previously unknown, was now rife, the

Barolong being specially infected. He believed in infection by cow's milk. Cows should be continually tested. Simple tracts had been distributed at Bloemfontein.

Dr. Dee related some cases of remarkable recovery after hopeless prognosis. Milk and fresh air accomplished many things.

Dr. W. G. Rogers complimented the Cape Town profession upon having accomplished more than had been done in any other place. The profession was perfectly well aware of the dangers, but the State neglected its duty. He alluded to the loose way in which physicians in England sent patients out to South Africa without any regard to the locality or the suitability of the case. The great difficulty was that patients had to earn their living. You could not compel them to go to a sanatorium. The duty of the profession was to rouse the State.

Dr. M. C. Hopkins dwelt upon the danger to Cape Town from the number of native servants in his district who went to that City and carried tubercular infection into families. He pointed out that the smaller municipalities were absolutely unable to tackle the question. He instanced a case he found in Britstown district some twenty years ago on a farm, the disease being absolutely unknown then in the district, but he supposed the patient must have been infected by some person who had come from a distance. Careful supervision of servants was most important.

Dr. Ireland, speaking as Mayor of Cradock, dwelt upon the utter financial inability of municipalities to deal with this problem, but hoped that a round dozen or so of medical men in Parliament would have no difficulty in persuading the State to step in and establish sanatoria in connection with which compulsory legislation would be required.

Dr. Mulvany gave some simple instances of apparently favourable results being obtained from certain farm remedies. The "Guaiacoles" were also used in Fraserburg.

Dr. Boyd dwelt upon the important factors of housing and sanitation, which had brought about an immense fall in the death-rate. The fact of the disease being much more prevalent amongst coloured people was largely due to their lower sanitation. If municipal bodies would get rid of the idea that any sort of sanitation was good enough for the coloured man, we should get some results.

Dr. Osler emphasised strongly the danger of house infection. All advanced cases should be segregated under strict medical supervision. Cheap segregation colonies could be arranged. He also insisted on strict sanitary inspection of houses.

Dr. Jasper Anderson supported the resolution for the Government Commission. The only hope of the movement succeeding lay in continuous agitation. The most important desideratum was improved methods of diagnosis in the early stage, which would result in a large percentage of permanent cures. Hæmoptysis was often the earliest, with no physical signs. He found Calmette's test reliable and without bad effects, but had discontinued Von Pirquet's He agreed with Dr. Marais as to the efficacy of Von Pirquet's method in detecting a latent focus. He was in the habit of using the old tuberculin in small doses simply to satisfy sceptics, who often knew all about it and were not satisfied till it was applied. The T.R. he used very seldom, as it complicated the problem of providing graduated exercise by the interruptions caused by rise in temperature. There was great trouble in finding a suitable form of exercise. White people objected to actual work, and the only thing you could prescribe for them was walking. There was no necessity for large and expensive sanatoria, but a large institution was in many ways much cheaper. They wanted merely administrative buildings and dining room and the cheapest possible form of dwelling for the patients. At Cape Town he used iron rooms lined with matchboard, but there was a difficulty in keeping them sufficiently cool in the summer-time. So far he was diffident about speaking of his results, but they certainly appeared to be quite good, and he took any class of case, slight or advanced, that was likely to benefit, and in some cases the recoveries were remarkable. So far he had had no time to go into the matter of mixed infections, but in some cases of high temperature he had used antistreptococcic serum with excellent results. The practical difficulty of getting patients into his chalets was that the early cases often could not afford to go, and if they could afford it, did not see the necessity whilst they were still early. He had five chalets and ten beds. He had never been able to keep them full, usually having six or seven people. He anticipated a good effect from the discussion before this Section, for there was a good deal of medical indifference to consumption.

Dr. Caiger dwelt upon the rapid extension of the disease in his district amongst a typical Boer population who had been born and bred there. People required to be educated to the value of fresh air, particularly school-teachers who kept the windows of the stuffy schoolroom closed. The worst offender he ever knew in this respect was a science teacher. He warned his brethren against the dangerous practice of not frankly communicating to tuberculosis patients the condition they were in. The pendulum had perhaps swung too far against the influences of climate and heredity, but they certainly saw the influence of climate here, and be believed that heredity at any rate accounted for a general constitutional predisposition, which we should take into account in the upbringing of children.

Dr. H. Clarke, as a District Surgeon of twenty-two years' standing, dwelt upon the rapid increase of the disease in Simonstown and spoke of the utter impossibility of local authorities stamping it out. Compulsory segregation under Government auspices was the only hope.

Dr. Gow spoke highly of the great value of the opsonic index in early stages. Tuberculin treatment was positively dangerous unless controlled by continual observance of the opsonic index, but otherwise was most valuable. Auto-inoculation was a blind force which they could not gauge or control, and which had been going on for centuries without results.

Dr. Porter pointed out the danger of the establishment of sanatoria attracting a great crowd of poor consumptives from oversea. In Johannesburg every notified case was sought out and instructions left with them, and, if they permitted it, disinfection was carried out. In the course of his inquiries there, he had found that the proportion of cases originated in South Africa was considerably greater than those imported. Back-to-back houses were a considerable danger. Although the tuberculosis death-rate of Johannesburg was lower than in Europe, the proportion amongst natives was absolutely appalling. The main crux connected with native phthisis was the prevention of spitting, but this was impossible in the mines. They are removed from mine work as soon as they are discovered to be phthisical. The white miners ought to be similarly removed, but they staunchly resisted this on account of the high wages they had. Compulsory isolation of consumptives was legal in New York.

Dr. M. Hewat said that the danger of attracting oversea patients to local sanatoria was a fallacy. All tuberculous immigrants could be kept out, or, at least, no one need be admitted to the sanatoria unless they could prove residence in South Africa for a certain number of years. The bedrock of the matter was the teaching of hygiene in schools. Teachers were often taken on when known to be consumptive.

The President said that the resolution was a hardy annual, but there was no harm in continuing to bring it forward. In Grahamstown the native mortality was appalling, but it was utterly impossible for any Town Council, and he feared utterly impossible for any Government, to go in for the policy of segregation. He might be pessimistic, but he thought the only hope lay in the gradual weeding-out of the susceptibles.

Dr. Murray Gray said that there was little for him to say in reply, as the discussion had entirely diverted from treatment, which was the subject of this paper. Sanatoria should be cheap, and the point raised by Dr. Porter should be looked out for. The dispensary system in England and as now run in Cape Town was very hopeful. He would put in the way of treatment food and hygiene first, exercise next, vaccines last. He related the case of a seaman who did admirably under graduated exercise, and whose case was utterly set back by injudicious vaccine treatment.

II.—A NOTE ON THE DOSAGE OF DIPHTHERIA ANTITOXIN.

By F. T. Waldron, M.B., B.S. (Lond.), Mossel Bay.

Diphtheria is admittedly very prevalent in this country—a good deal more so than is shown by the notification returns. It would be interesting to know how many cases arise year by year that are not seen by a medical man—in the towns perhaps not a great many, in the rural districts probably a large number. It is not an uncommon experience to be called some distance to a case of diphtheria, or to see a case brought from a distance, and to find on enquiry that sore throats have been suspiciously prevalent in the neighbourhood.

There are several obvious factors that tend to keep the disease from coming to light easily in places where medical attention is not easily obtained. It is largely an ailment of the poorer classes; it passes over or ends fatally fairly quickly; it is easily confounded with troubles that are not dangerous enough to demand the luxury of a visit from the doctor; and it may pass unrecognised in a mild form or be present with symptoms different to the popular idea of the disease, notably the nasal form. In poor houses a child with nasal diphtheria is usually up and about the whole time, spreading infection broadcast.

Amongst these poor families one occasionally sees a condition looking at first sight like an aphthous stomatitis that would probably in many cases prove to be due to the diphtheria organism.

I was much struck recently, in visiting a kloof about twenty miles from Mossel Bay, by the gradations to be seen in children of the one family at one moment from what one would call by appearances follicular tonsillitis to undoubted diphtheria. There were eight children in the house, in age varying from two to twenty years. One, aged 5, had undoubted diphtheria, and died of heart failure next day. All had greatly enlarged tonsils. Two had nasal discharges, and in two the appearance of the tonsils suggested simple follicular tonsillitis. Bacteriologically, probably the whole family would have given diphtheritic growth from the tonsils.

Being on a visit to the kloof at the instance of the Divisional Council, I called at about half a dozen other poor houses near. The invariable presence of greatly enlarged tonsils made one realise how completely the soil is prepared for the seed when it comes along. Presumably the reason for the almost invariable tonsillar trouble amongst the children is the poisonous air they sleep in at night, when large families crowd into cramped quarters with all doors and windows closed. Terrible overcrowding with all its evil effects is found even more surely in the open country districts than it is in the slums. The ineradicable tendency of the poor whites to crowd into each other's houses at times of illness is a serious factor in the spread of such diseases as diphtheria.

The free-and-easy method of death certification, in the absence of any medical attendance, that obtains in the rural areas is by no means certain to bring to light even all the fatal cases. One suspects that not a few of those under the suspiciously common heading of bronchitis may be laryngeal obstruction cases.

Cut-and-dried as the antitoxin treatment is now-a-days, after our fifteen years' experience of it, we must by no means take it for granted that the knowledge of it has spread thoroughly amongst the general public. One is being constantly surprised to find how the fact that we have such a potent remedy wherewith to attack the disease has failed to reach the less enlightened sections of our community, and it is evident that there is need for us individually to do what we can to educate in this matter, so that the benefits of its early use may not continue to be lost as often as it still is. Many farmers, for instance, who can discourse wisely in the matter of curative and preventive inoculations in cattle seem to be remarkably ignorant of our efforts in this direction for human ailments.

Any criticism as to the results of treatment would of course be best supported by actual figures such as are compiled at the large infectious disease hospitals. Here we are given the mortality of the disease in cases treated from the first, second, third or any later day, and such tables show in the most striking manner how Behring's original prophecy is born out in the diminishing power of the antitoxin the later its use is begun. For private practice here, as elsewhere, needless to say such valuable figures are not available.

To arrive at some approximate idea as to the case mortality of the disease we may take, in any period, the notifications by medical men and the deaths certified by them. Through the courtesy of the various Medical Officers of Health for the Provinces I am able to give these. In the Cape Province returns from the ninety-three chief towns only are considered. In Europeans during 1909 there were 371 cases of diphtheria notified and 71 deaths, a mortality of between 19 and 20 per cent.; in Coloured, 101 notifications and 38 deaths—this, with a mortality of 37 per cent., so obviously means that only the worst cases were seen that the figures will not be further considered.

In the Natal Province during 1909, considering only Europeans: 111 notifications and 17 deaths, a mortality of about 15.5 per cent.

In the Orange Free State notification is not sufficiently general to allow of any deductions.

In the Transvaal during 1909 there were 143 cases notified and 46 deaths, a mortality of 32 per cent.

Obviously these figures cannot be supposed to give anything more than a very rough idea as to the results of treatment; still they are certainly not so good as to make one feel that it is quite unnecessary to review what is the main factor to be looked to for producing an improvement in them. The antitoxin treatment has been available now for about fifteen years, so that plenty of time has elapsed for the accumulation of information. The whole thing has taken shape for most of us in the course of our work in practice, and each of us has his own idea and procedure in the matter. We know pretty well what will be the dose used of most things by our medical confrères, but I think a good deal of variation would be found in the use of antitoxin.

The results of the treatment, marvellous in particular cases as they are, depend so greatly on the time factor that there does not seem to be much room for discussion as to the dosage in the very early stages; still there are points to consider. The remedy is provided for us from the laboratory in definite strength and small bulk. No opinion or advice is given as to the dose. As, up to the present, laboratory methods, even where such control is available, will not show in any particular case just how much poison there is to be neutralised, the rest is left to us. Culture experiments show that there is no doubt as to the remarkable variations in virulence in different strains of the organism, and presumably also in the rapidity of toxin formation.

The soil varies as well as the seed, and we are all familiar clinically with the type of child and the type of naso-pharynx that may be expected to encourage rapid growth. So that even in the very early stages we cannot be entirely confident in regarding one size of dose as available for all, unless—and this is the important point,—the dose is to be big enough to cope with the least favourable early conditions. The age and bulk of the patient seem to signify less than anything else, and except in the case of very young children need hardly be taken into consideration.

For a couple of years I had, in England, charge of a small isolation hospital in a rural area. We had about fifty cases a year of diphtheria. They all seemed to be sent in pretty early. Many were given 2,000 units before being sent away by the doctor certifying. Pretty well all were given 4,000 units on admission.

Laryngeal cases did not arise, and the results generally seemed to indicate that these small doses were enough.

My impression is that in this country the disease is almost always of a more virulent type. 6,000 units I believe to be the very smallest initial dose to be satisfied with in any case, however mild and however early, with another 6,000 in six or eight hours if the membrane is not quickly loosening off and clearing, however little there may be of it. If the membrane is at all extensive when first seen, however early the case may be believd to be, 10,000 or 12,000 units is the dose suggested. Where there is a good deal of membrane and the disease is known to be in the third or fourth day, 20,000 units should be given at the outset.

The attitude that I think we ought to take up is that this is about the truest remedy in the whole domain of therapeutics. We know what an innocent beginning used to develop into in pre-antitoxin days, and how the disease progressed in spite of all treatment. We must feel as we use this stuff that at least in one direction we in this generation are privileged to touch the ultimate goal of scientific treatment. We are still condemned to grope about with drugs in so many directions, but in this matter and with this invaluable agent it behoves us to take nothing for granted as to the limitations of its powers, and to make certain that at stage by stage of the disease we are using the remedy so as to gain its full effect.

Theoretically, although time is such an important factor, no case of diphtheria should be too late to be benefited by such a true antitoxin, if enough of it be given. It is really bad late cases where the free use will show what marvels may be worked.

The part of the subject I wish more particularly to raise is the dosage in bad cases, with very extensive membrane filling the fauces, or poisoned with toxin, or those in which there is laryngeal obstruction. Laryngeal are certainly not necessarily late cases, as in so many children the larynx seems to be invaded from the first, there being often no membrane visible in the throat. The remedy, so far as we can see, is harmless, or even beneficial apart from its specific action. The rash and arthritis even seem less common than they used to be. I remember one practitioner of the older school who was a firm advocate of the antitoxin treatment, a late event in his career, but who said he was sure that heart failure pure and simple carried off more cases than under the old conditions. I am pretty sure this is not confirmed by general experience, but one can believe that now so many more cases recover that a relatively uncommon cause of death may become relatively more common. I have never been able to satisfy myself as to any ill effects from the serum, however large the dose, and it is one of the points on which one would like to hear of any experience to the contrary.

Everything points to the fact that so far as ill effects go we must not hold our hands. Avoidance of needless waste of the material and disturbance of the patient are the main considerations, and neither of these is very serious, as the serum is easily obtained and the injection of large quantities does not mean repeated puncture if a large syringe is used.

Whenever we lose a case, especially one seen late on, it is suggested that our attitude should first be one of doubt as to the sufficiency of the antitoxin used, and not of philosophical resignation at the foolish delay in calling the doctor. To inject early is the best of advice, but at any rate in country practice we cannot by any means always inject early; it is perhaps more often late than early. If it is a case at a distance and not likely to be seen again, if the case is at all late, or there is some definite laryngeal obstruction, an injection of 30,000 or 40,000 units would be the safest plan. If the case can be seen again in six or eight hours, 20,000 or 24,000 units should be given at once and repeated if indicated.

Can anyone say with confidence what dose is so high that it is no use going beyond it? That is the particular bit of information that would be very helpful.

With regard to laryngeal cases, it is here that I think the powers of large doses may be most striking. Laryngeal diphtheria in which there is ever present doubt as to the advisability of urging tracheotomy is certainly one of the bugbears of our private practice. Tracheotomy can be, and often is, a distinctly uncomfortable little operation under bad conditions and with a certain type of child. My own opinion is that in the poorer houses, where it is so often wanted, without skilled nursing it is hardly worth doing. I have an idea that our opinions as to the need for tracheotomy and the stage of obstruction at which to do it are still too much tinted by experience gained in pre- or early antitoxin days. One should regard it now quite as a last resort. 24,000 units repeated two or three times with stimulants and expectorants such as ipecacuanha and senega in frequent small doses may give as good or better results than a rather early tracheotomy. Difficulties with the tube, suppuration and septic broncho-pneumonia, the poisoning and depressing action of chloroform, or the straining against being held where it is done without an anæsthetic, are all real enough dangers.

I have recently watched with great interest two or three cases treated by my partner, Dr. C. Kitching, whose custom I follow in urging large doses. In the first tracheotomy was declined by the parents, and in the others not urged. In one 12,000 units every four or six hours, and in the others a couple of doses of 25,000 units worked marvels, and recovery was gradual but uninterrupted. Doses of 6,000 or 8,000 units would probably have been useless according to my previous experience. It would not take many such cases to make one give up tracheotomy in diphtheria altogether; one's inclination is to give plenty of antitoxin and let the child fight it out.

Opportunity arises not infrequently of using the serum as a prophylactic. The protection probably does not last more than two or three weeks; still this is obviously very useful to help to tide over a time of particular exposure to infection such as takes place in big families in poor houses. Doses of 500 units are mentioned in this connection, but no doubt most of us use the convenient dose from a dispensing point of view of 2,000 laboratory units.

III.—THE CLINICAL VARIETIES OF MALTA FEVER MET WITH IN SOUTH AFRICA.

By A. Garrow, M.D., Steytlerville.

This paper is founded on an experience of Malta fever extending over three years, during which time I have been in practically daily contact with the disease or its sequelæ. I shall have occasion, however, to refer to cases which I met with in my practice in the same district during the four years prior to the above period. These cases were at the time undiagnosed. During last year I had the valuable, although unpleasant, experience of having passed through an attack of the acute one-wave type.

It gives me pleasure to acknowledge that it was the papers in the *South African Medical Record*, of my former fellow-student, Dr. P. D. Strachan, of Phillipolis, that lead me to discover that Malta fever exists in this district.

Interesting papers have appeared from time to time in the same journal from the pens of Lieut.-Colonel Birt and Major McNaught, of the R.A.M.C., and Drs. J. E. McKenzie and D. M. McRae. That of the latter deals particularly with points in diagnosis.

I read Dr. Strachan's first paper in 1904, but it was not until 1906 that I diagnosed my first case. Subsequent experience has taught me that I had encountered cases between 1904 and 1906, but had failed to diagnose them, even although I was actually on the alert for the disease. The reason for this was that after reading Strachan's first paper, I read up the text-book descriptions of the fever and kept on the outlook for a case conforming to that description, and, as I have said, it was not until 1906 that I first met with such a case. I relate this personal experience for the benefit of those practising in the goat, and therefore probably infected, districts of South Africa, who may think, as I did during these years, that they have not seen many cases of the disease. I did not know then what I know now, namely, that most of the cases we meet in this country are of a very chronic nature and run, practically speaking, an afebrile course, and that it is only occasionally that one meets with the typical undulant cases. I should say that in South Africa the chronic type is endemic, and that we meet with acute and undulant cases sporadically, and sometimes in epidemics.

The extraordinary variability in the behaviour of the disease may be gathered from the fact that the monograph of Hughes contains as many as eighty pages devoted to a description of the symptomatology and clinical manifestations and combinations met with. Any less complete description is inadequate for those of us who are in almost constant contact with the disease. Unfortunately the book is out of print. The clinical part of it is so excellent that one hopes that someone will think of undertaking the preparation of a new edition now that the true etiology has been discovered. A new edition is certainly needed, for of late years it has been found that the geographical distribution of the disease is much more extensive than was formerly thought. One of the latest additions is far-off Uganda. Sir David Bruce, in his speech at the last annual meeting of the Research

Defence Society, stated that the Sleeping Sickness Commission, when in Uganda, had, in addition to their work on sleeping sickness, established the fact that the disease of which thousands of natives in a certain part of the country were sick, and of which more than a thousand had been reported as having died, was Malta fever.

Much information, as well as a *résumé* of the symptoms, is given by Eyre in the Milroy Lectures for 1908, which are published in *The Lancet*.

It must be remembered that I write this paper from the up-country practitioner's point of view. He is generally placed in a most difficult position with regard to his country cases. He is called to see a case on a distant farm, he gets a history which is not always clear and reliable, and from it and one examination of the patient, he has to make his diagnosis and give a prognosis. Thus a point in a case once missed is missed, perhaps for good. It behoves him, therefore to be particularly on the alert for all the possibilities of a given case.

The question of the diagnosis of Malta fever is also of importance to the coast practitioner. Many cases go to the coast, some, perhaps, dissatisfied with the results of the treatment up country, and hopeful that there may be some truth in the prevailing idea that a change to sea air effects a cure; while others go to the coast well, and while there have a relapse.

SYMPTOMATOLOGY.

Hughes begins his chapter on symptomatology with the following words: "So varied are the symptoms, and so uncertain the duration and course of this fever, that it is impossible to give a description to which all cases can be referred."

The variability of the symptoms precludes the possibility of attempting any systematic description of them individually and in detail in a paper like this. In the remarks which I shall make on the different types of the fever, and in the examples which I shall give, most of the symptoms will be referred to, but the purpose of this paper is to indicate the clinical varieties we meet with in South Africa, and not to attempt any exhaustive description of the symptoms.

Tongue Appearances.—The appearance which the tongue presents in this disease, however, is a subject upon which I should like to dwell in detail. I entirely agree with what Drs. Strachan and McRae have said as to its importance in diagnosis. I have repeatedly found that the condition of the tongue has helped me to a correct diagnosis in obscure chronic cases presenting no other symptoms than those associated with a general breakdown in health: and, as I shall mention later, its appearance in the acute cases often helps to differentiate the disease from enteric fever. To begin with, my experience is that in the great majority of cases the tongue of Malta fever is a moist one. It is usually large and flabby, and presents lateral indentations from the teeth. The edges and the tip are red. It is the condition of the fur on the dorsum, however, that presents the variations which, as Hughes says, are the peculiarities of each and every symptom of the disease. The most common condition is that one finds over the dorsum a white, glistening, moist coat of varying thickness, with pieces of viscid frothy saliva adhering here and there. The glistening, sparkling appearance is sometimes very striking. Another common appearance is a thick, white, moist coat completely up the papillæ, but with none of the glistening appearance and with no specks of white frothy saliva on it. The tongue sometimes looks as if a thick streak of white lead had been laid along the dorsum. I think that it is most suggestive of Malta fever if, in a case of chronic ill health, one finds a tongue corresponding in appearance to the conditions described above; and one should at once be led to examine for other signs and symptoms of the disease, and if they are not sufficiently marked to enable one to make a definite diagnosis, the blood may be submitted to the Widal test.

I have said that these tongue furs are white. The tongue coating of Malta fever is, in my experience, most often white. One sees sometimes a fur of extraordinary whiteness. However, in some cases the fur may be brown, and, as far as my experience goes, when one gets a brown tongue fur in chronic cases it is generally very thin. The condition looks as if the enlarged papillæ had been singed. In acute and in malignant cases one may have the dry brown cracked tongue usually associated with enteric fever; but even in these types of the disease the tongue often remains quite moist. In fact, in one of the two malignant cases which have come within my experience the tongue remained white, moist and glistening throughout, although the temperature for the last few days ranged between 102° and 104°, and reached 108° before death.

Temperature.—Hughes's hospital experience of the disease has enabled him to give a very detailed description of the variations in the types of the temperature curves, and he gives many illustrative charts. It is seldom possible, under the conditions of South African country practice, to make such detailed temperature observations. I include with this paper two temperature charts from my own practice, one that of a malignant case (Fig. 1), the other that of a case of the one-wave type (my own attack, Fig. 2).

Other prominent symptoms of the disease which I shall no more than mention, are profuse perspirations, headache, pain at the back of the neck, pains in the joints with or without swelling of them, pain along the course of nerves, pain along the backbone, and especially in the mid-dorsal region, obstinate constipation (although less commonly there may be diarrhœa), anorexia, epigastric fullness, tenderness on pressure in the epigastric and in both hypochondriac regions, insomnia. One should lay special emphasis on the fact that the poisons of this disease seem to have a selective action on the nervous system. There is during pyrexia great nervous irritability, and after the pyrexia and in cases mainly apyrexial we get great despondency. Orchitis has been rare in this district.

Enlargement of Spleen and Liver.—In an analysis of the clinical symptoms in one thousand cases of Malta fever, Eyre puts enlargement of the spleen as having occurred in fifty-one per cent. of cases, and enlargement of the liver in eight per cent. My experience in this district would put the latter at much higher percentage, and especially in chronic cases and in young subjects, children particularly.

CLASSIFICATION OF TYPES OF THE DISEASE.

I have given a considerable amount of thought to the question of a classification which will bring into prominence certain types of the disease which would appear to be more common in South Africa than elsewhere.

I think that every case that one is likely to meet will find a place in one or other of the following groups:—

I. *Acute type*—(*a*) Malignant Cases; (*b*) One-wave Cases; (*c*) More-than one-wave, or the Undulant Cases. II. *Subacute Type.* III. *Chronic Type.* IV. *Complicated,* including those cases which first come under observation on account of a complication or sequela of Malta fever, or during a concurrent illness.

The clinical manifestations in a case of, say, the chronic type are so entirely different from those found in one of the acute varieties that, but for the Widal test, it would be impossible to believe that both patients were suffering from the same disease.

There is a connection, however, running throughout all the varieties, and the best conception of that is gained by remembering that the undulant type is what might be termed the central type. All the other types are but variations of it, depending upon varying combinations of such factors as the quantity of the virus and the virulence of it, and the state of the defence forces of the patient.

The undulatory type is characterised by "intermittent waves or undulations of more or less remittent pyrexia of variable length and number, separated from one another by periods of variable length of temporary abatement or absence of symptoms."

The acute one-wave type designates those cases characterised by simply one wave or undulation of varying length and severity, followed usually by a convalescence and recovery.

The malignant type includes those acute cases characterised by great severity of symptoms and profound toxæmia, and generally resulting in death.

On the other side of our central type are the subacute and the chronic, in which, owing, perhaps, to a weakened virus, or a smaller quantity of it, or to more sluggish action of the defence forces of the body, the pyrexia is more or less subdued.

The cases which go to form the complicated type are in the majority of cases subacute or chronic. I have made a special type of it because of its importance in the conditions of South African country practice.

This classification is, of course, more or less artificial, but it is nevertheless useful both for purposes of description and for a better conception of the disease as a whole. Thus in a chronic case the fever may for some reason burst into flame and the case become of the acute, malignant, or acute one-wave types, and this wave of pyrexia may be but the first of several, and the case would then be classed as undulatory.

DESCRIPTION OF TYPES.

I. Acute.

(*a*) *Malignant.*

The onset is usually fairly sudden, and the initial complaints are of some or all of the following symptoms: intense headache, severe pain in the muscles at the back of the neck, general pains, shivering fits, nausea, anorexia, and great thirst. The temperature remains high, the face flushed, the pulse rapid; the tongue is furred, and usually moist and white, although it may become dry, cracked and brown; the mind is often wonderfully clear; there may or may not be abdominal tenderness and distension. Abdominal tenderness, when present, is most common in the epigastric and hypochondriac regions. There is usually some degree of splenic enlargement, and also sometimes of hepatic. Constipation is the rule, with very pale motions. Pulmonary catarrh passing on to lobular pneumonia is very common. If there are remissions of the temperature, profuse perspirations accompany them.

The course of the case is usually one of increasing toxæmia, pyrexia, and rapidity of pulse, and the termination usually death.

The diagnostic difficulty is in differentiating from typhoid fever. Points in favour of Malta fever are moist tongue, rapid pulse, clearness of mind out of proportion to the gravity of the case, profuse persirations; but when the tongue is dry and cracked, the patient delirious, and the motions loose and pale yellow, a certain diagnosis cannot be made on clinical grounds alone, and recourse will have to be had to the Widal test.

EXAMPLE.—*Case No. 1: Male, aged 22.*—The patient first came under observation on January 9th, 1906, with the following history. About fourten days previously he had felt " full " about the stomach, and he took a purgative and felt better. On New Year's Day, that is, ten days before, he felt bad again, and on taking his temperature in the evening, he found it to be 101.8°. He, however, kept moving about, although feeling very ill, until the 4th, when he took to bed. On the 9th he was brought into town on a wagon. His main symptoms were profuse perspirations, some general pains, and a " done-up " feeling, especially between 10 a.m. and 3 p.m. There was scarcely any headache, the appetite was gone, he had a bitter taste in the mouth, flatulent pains, and constipation. His face was not flushed; the tongue was of the moist, white glistening variety. The edge of the spleen was palpable. No abdominal tenderness. The pulse was regular, fairly easily compressible, and its rate 116 per minute, the temperature at the time of this observation having been 103.8°. The chart of his temperature from the 10th to the day of his death, six days afterwards, is shown (Fig.1). The temperature fell only once to below 102°, and ranged mostly between 103° and 104° On the 16th, hyperpyrexia set in, and a temperature of 108° was registered before death, in spite of the many measures which were tried to lower it.

During these six days the general appearance of the patient was that of increasing toxæmia, but he remained quite conscious when spoken to until the day before death. His tongue remained white and glistening and perfectly moist. His abdomen became very distended during the last two days, and on the day of his death he had incontinence of very foul-smelling, clay coloured, soft, unformed motions. During these last three days the pulse-rate was about 140, and on the day of his death it rose to 160 per minute.

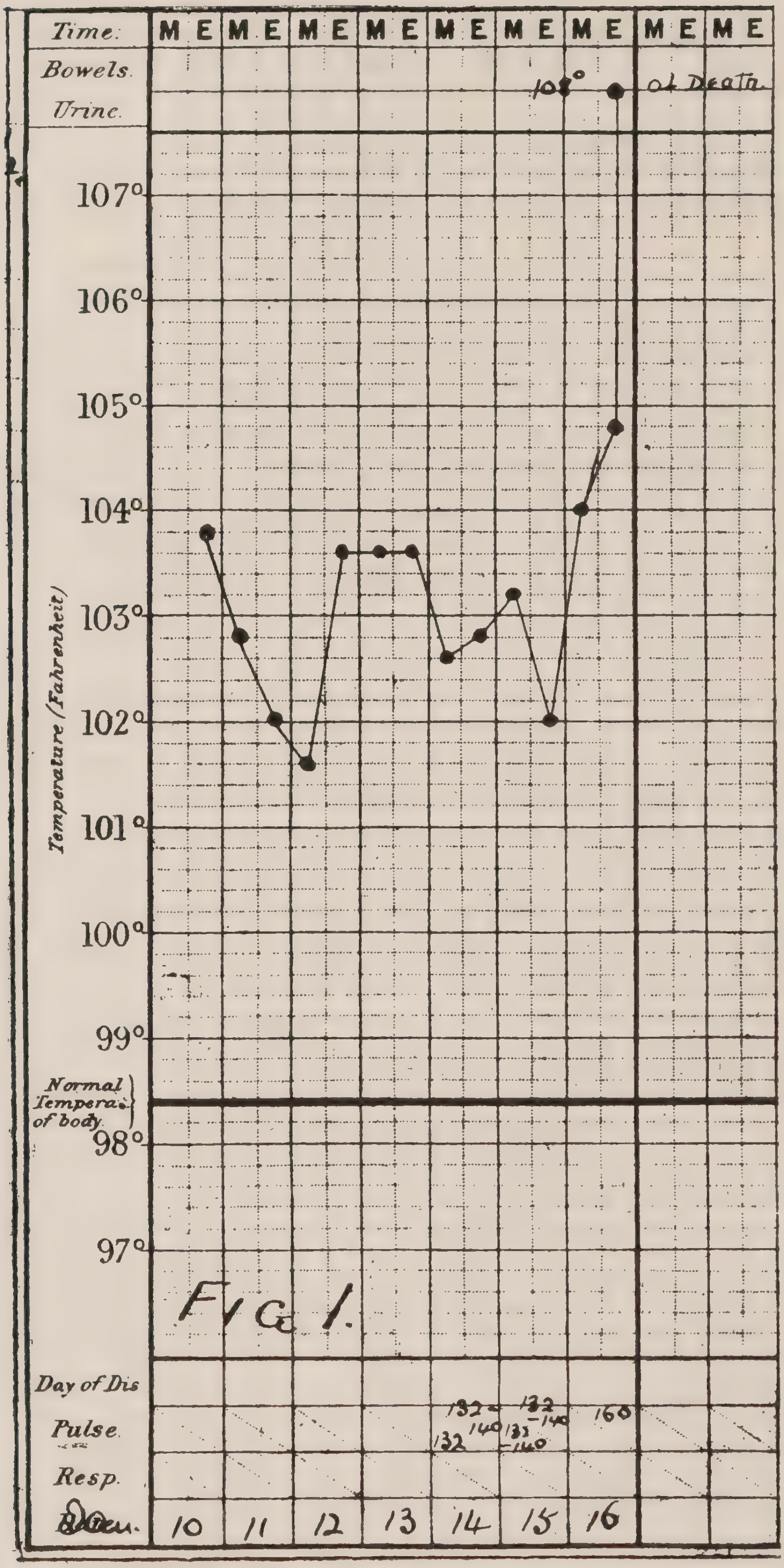
Time.
M E M E M E M E M E M E M E M E M E
Bowels.
Urine.
108° at Death
Temperature (Fahrenheit)
107°
106°
105°
104°
103°
102°
101°
100°
99°
Normal Temperature of body
98°
97°
Day of Dis
Pulse.
132 140 132 132 140 132 140 160
Resp.
Dec.
10 11 12 13 14 15 16

Fig. 1.

(*b*) *Acute One-wave Type.*

The symptoms of this type are somewhat similar to those of the one just described, but are of much less severity, and the temperature is more remittent. The case is very like one of typhoid fever, but some of the following characteristic symptoms and signs are usually present: pains at the back of the neck, profuse perspirations accompanying the remissions of temperature, nervous irritability, a tendency to insomnia, neuralgic pains, pains in the joints, sometimes accompanied by effusion into them, a tendency to bronchial and pulmonary catarrh, and the white, moist glistening tongue. The mental condition of the patient is generally much clearer than one finds in a case of typhoid of similar severity. The duration of the pyrexia and its manner of subsidence are similar to what we are accustomed to in typhoid fever.

I have seen many cases in which there has been only one wave of pyrexia. Such cases cannnot be called undulatory; therefore I place them in a class by themselves. They are particularly liable to be wrongly diagnosed as typhoid fever, and one's suspicions as to the correctness of the diagnosis are not afterwards aroused, as they would be if wave after wave of pyrexia followed, as in the true undulant type.

The chart (Fig. 2) shown is that of the temperature of my own attack. It looks exactly like the chart of a case of typhoid fever. The onset in my case was abrupt. I had rigor on going to bed on the night of April 1st, 1909. Next day I awoke with a severe headache, which, however, left me during the forenoon. During the afternoon I felt continuously shivery, and on the third day I took to bed. The observations on the chart begin on that day.

(*c*) *Acute More-than-one-wave or true Undulatory Type.*

This appears to be the most common type in the Mediterranean, but is not so here.

The onset is more insidious than that of the two types just described, and is characterised by such symptoms as low spirits, nervous irritability, insomnia, anorexia, nausea, headache, pain at the back of the neck, and a "creepy" feeling up and down the back. There is usually some time of the day during which the patient feels fairly well, and that is generally the forenoon. The patient does not feel ill enough to take to bed, and continues at work. After a variable time, extending to several weeks, although it may be even months, he is forced to go to bed, and he goes through an attack similar to that described under the one-wave type. A few days, however, or even one day after the temperature has subsided it again begins to rise, and a relapse ensues, and this may occur again and again. Hughes says that the pyrexial duration of this type may extend to three hundred days or more. The anæmia, muscular wasting and weakness which result from a succession of relapses may be extreme, and when there is much pulmonary catarrh the picture presented is that of the last stages of phthisis. Unlike the patient in the latter condition, however, the Malta fever patient in this stage is extremely despondent about his condition. Finally, however, the pyrexial waves cease and the tongue begins to clean, and convalescence begins. Neuritis, joint pains with or without effusion, dyspepsia and nervous palpitation are particularly common for a long time after recovery from this type.

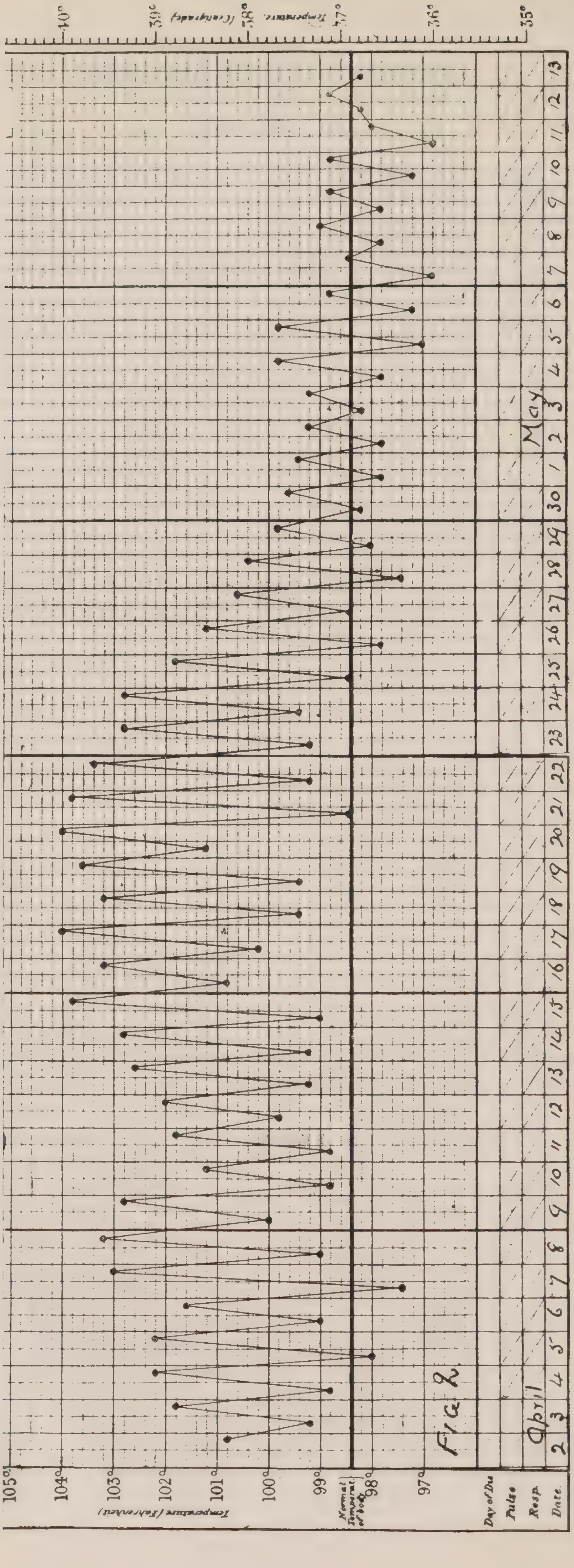
105°
104°
103°
102°
101°
100°
99°
Normal Temperat. of body
98°
97°
Temperature (Fahrenheit)
Day of Dis
Pulse
Resp.
Date.
40°
39°
38°
37°
36°
35°
Temperature (Centigrade)
April
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21
22
23
24
25
26
27
28
29
30
May
1
2
3
4
5
6
7
8
9
10
11
12
13

Fig 2

The temperature chart of a case of this type illustrates well the definition which I have already given of this type of pyrexia, which is slightly amplified from Hughes, namely: "It is characterised by intermittent waves or undulations of more or less remittent pyrexia of variable length and number, separated from one another by periods of variable length of temporary abatement."

II. SUBACUTE UNDULATORY TYPE.

In this group are placed those cases which are considerably milder than the type described last, and in which the periods of apyrexia would seem to be longer than those of pyrexia. In general practice, however, in this country, one has little opportunity of making definite and prolonged temperature observations in cases of this type. Country patients are usually seen during the apyrexial period, and are generally not ill enough during the pyrexial period to think of sending for a doctor; in fact, during the pyrexial period the patient may not even be confined to bed, although the afternoon temperature may be as high as 103°, or perhaps over it. A very usual occurrence is that after the first pyrexial wave has subsided the patient comes in from the farm with a ready-made diagnosis of influenza, and thinks that a tonic will put him right. If the true significance of such a case is not at once recognised, and appropriate treatment not instituted, the patient will return to his farm and find that this attack is probably the first of many, and a more or less prolonged period of ill-health will ensue, with varying combinations of symptoms, complications, and sequelæ. A careful enquiry will elicit information about the premonitory stage of symptoms already referred to, and an examination of the patient will perhaps reveal suggestive tongue appearances and splenic or hepatic enlargement. But there is nothing like submitting a few specimens of blood from cases of this type, and of the chronic type, to the Widal test, to make one realise how many cases there are of this kind, and how often one has missed them in the past. All blood examinations from my cases have been performed at the Government Bacteriological Laboratory presided over by Dr. Robertson, to whom I express my thanks for the help obtained.

III. CHRONIC TYPE.

It is fairly common to come across cases presenting varying combinations of the usual symptoms and signs in people who still continue at their ordinary occupations throughout the whole trouble, and who do not seem to have any pyrexia at any time, although one cannot make any definite statements on that point, because prolonged and frequent temperature observations are not made on these patients. Cases belonging to this type may be missed under such diagnoses as dyspepsia, anæmia, neuralgia, refraction headaches, chronic rheumatism, general debility, etc., etc.

IV. COMPLICATED TYPE.

This group is formed to include those cases which have developed some complication the symptoms of which are the predominant features when the patients *first* seek advice, also those chronic Malta fever patients who are seen for the *first* time during a concurrent illness.

The subacute and chronic types are by far the most common in a district like Steytlerville, and they are the types which furnish cases coming for the first time under observation on account of a complication, a sequela, or a concurrent illness.

It is of the greatest importance to recognise the Malta fever element in these cases, so as to be the better able to give a more reliable prognosis. If this is given without taking into account the Malta fever element, and simply on what we know of the usual behaviour of the given complication when it occurs as a primary disease, later events are not likely to turn out always according to predictions. In these cases the predominant symptoms are those of the complication, whatever that may happen to be, but in all of them a close enquiry will reveal that there was a previous period of ill-health with the usual symptoms in varying combinations, and it is only by a concentration of attention on these symptoms, and following up the clues which they give, that one is led to examine carefully for Malta fever signs and symptoms, and is saved from making an incomplete diagnosis.

When an attack of Malta fever follows immediately after some acute illness, it often causes anxiety and trouble, if undiagnosed. For instance, just lately I had a case of measles in a girl aged twelve. After recovery from the measles she developed an illness characterised by general pains, constant coughing, drenching sweats, remittent pyrexia, and the physical signs of broncho-pneumonia. One's first thoughts, naturally, were of that fatal sequela of measles, acute pulmonary tuberculosis. It turned out, however, to be a case of acute one-wave Malta fever of three weeks' duration. I wondered at the time if she had been infected by that abominable treatment of measles which is so frequently adopted, namely, swallowing copious draughts of a tea made from goat's dung.

I have chosen the following cases from my note-books as exemplifying the great variety of cases which belong to this group.

Pulmonary and Pleural Cases.

Case No. 2: Male, aged 50, Farmer.
July 8th, 1908.

I was called to see this patient to-day on his farm, thirty-two miles away, and was informed that some days before he had been seized with a severe pain in the right side aggravated by coughing. Mustard plasters and hot poultices had been applied, and had brought considerable relief. Further enquiry elicited the fact that his health had been distinctly deteriorating for some months. He had been getting emaciated, his appetite was gone, he had been becoming more and more unfit for his work; every afternoon he had complained of feeling chilly, and as he had a cough, he was generally supposed to be developing consumption. When I saw him he had an anæmic, thin, and haggard appearance, his tongue was thickly coated with a white, moist, glistening fur, his pulse was 90, temperature (afternoon) 101°, and respirations 20 per minute. Examination revealed dulness, loss of vocal fremitus and of vocal resonance, and a diminished tubular respiratory murmur over the lower half of the right side of the chest, back and front, and depression of the liver, which was palpable and tender.

The diagnosis of pleurisy with effusion was made, and it was thought to be secondary to Malta fever, and blood was taken, tested at the Government Laboratory, Cape Town, and found to react positively at a dilution of 1 in 100.

July 14th, 1908.—Seen on his farm to-day; practically no change.

July 19, 1908.—Seen again to-day. Patient's general appearance better, but the physical signs in the chest unchanged.

August 25th, 1908.—After an interval of five weeks I was called to the farm to-day. I was informed that he had been progressing fairly favourably, one day better and the next day not quite so well, until about four weeks ago, when, after a severe fit of coughing, he brought up a lot of very foul-smelling pus. Indeed, for days afterwards the smell in the room was such that his nurses could scarcely bear it. This expectoration of greenish-yellow pus has continued. Careful physical examination of the chest revealed no change in the physical signs, but it was thought that the pleural effusion had become purulent, and his removal to Uitenhage hospital was advised.

This advice was followed within a week, and he was there operated on, and a large quantity of pus was evacuated from the right side of chest. After that his progress was good, and some time later he returned to his farm with a discharging sinus.

The sequel was very unfortunate. In December of that year he returned to Uitenhage Hospital to have another operation performed, and died while being put under the anæsthetic.

Case No. 3: Female, aged 61, Farmer's Wife.
August 19th, 1908.

To-day I was called to the farm, twenty miles distant, and found her to be suffering from right apical pneumonia, with a profuse herpetic eruption on the lips.

Fortunately I had seen this patient shortly before, when she was suffering from severe pain in the left hip, of ten days' duration, and sometimes of agonising severity. The associated symptoms and signs were: haggard, thin appearance, thickly-furred, white, moist, glistening tongue, pain on deep pressure over the left gluteal region, general abdominal tenderness, a tender liver, the edge of which was palpable two inches below the costal margin, and a history of insomnia, constipation and sweats for some time previously.

The blood was tested at Cape Town, and found to react positively at a dilution of 1 in 500. The pneumonia terminated by crisis on the sixth day. I saw her the day before the crisis, when she appeared to be *in extremis*. She gradually and slowly improved after recovery from the pneumonia, but the hip pain persisted. During September I was informed that she had a relapse of the Malta fever symptoms, but I did not see her. Her condition remained in a very unsatisfactory state, but the family, who had seen a fair amount of Malta fever amongst their neighbours, considered that all her symptoms were due to that. On January 2nd, 1909, however, I was asked to go out to see her. Her symptoms were now severe, and constant pain in the abdomen, and a "lump" there. Examination revealed a large nodular, very tender mass filling up the right half of the abdomen down to the level of the umbilicus. I diagnosed it as malignant, and probably hepatic in origin, but I had only the one opportunity of examining it. She died three days later.

HEPATIC CASES.

Case No. 4: Male, aged 60, Farmer.
May 19th, 1908.

To-day I saw this patient on his farm, fifty miles distant. He had first taken to bed on March 28th, on account of pain under the right costal margin, which was fairly continuous in character, with acute exacerbations. He remained in bed for seventeen days, during which the chief symptoms were feverishness, severe headache, and sleeplessness, in addition to the symptoms associated with the hepatic region. He recovered somewhat, and got up, but had a relapse with similar symptoms. This lasted some ten days, and he again got up. Yesterday, however, he had a sudden seizure of this pain, and I was sent for. I saw him at 3 p.m., when the temperature was normal and the pulse 80. The tongue had the typical white, moist appearance. There was some conjunctival icterus. The liver was enlarged, smooth and tender, and its edge distinctly felt three inches below the costal margin. The urine was bilious and the fæces pale. Some blood was taken, which, when tested at Cape Town, reacted positively at a dilution of 1 in 25.

During the following week he was removed in a wagon by easy stages to the village, which he reached on 31st May. During that night I was summoned, and found him suffering from typical hepatic colic, which was subdued by opium. His physical condition was as noted on the 19th of the month, except that he was much thinner and markedly anæmic, and that he complained of pain in the right side and back, just above the hepatic region, where, on auscultation, some creaking friction was audible. From May 31st to July 18th he remained under constant observation in the village. During that time he spent the first five weeks in bed between blankets. He had no recurrence of hepatic colic, and his progress was continuous, but very slow. The enlargement of the liver slowly subsided, the motions and urine assumed gradually their normal colour, and the icterus disappeared. Towards the end of the period of convalescence, rheumatic pains without effusion supervened, and caused great discomfort. They were confined chiefly to the finger-joints, but at times the larger joints were also painful.

On August 10th I had an opportunity of seeing and examining him. His colour had much improved, as well as his strength, but he was still more or less a martyr to rheumatic pains. Physical examination was quite negative. On April 27th, 1909, this patient was brought into the village on account of severe pain in the cardiac region. I was then in bed suffering from Malta fever, and my brother saw him and found loud pericardial friction. Counter-irritants were applied and potassium iodide administered, and the condition soon subsided. Since then the man has had no medical attention. I see him often, and he looks hale and hearty.

Case No. 5: Female, aged 32.—The following case came under my observation in those days before I had learned that the variety of clinical conditions which may be met with in a case of *Micrococcus* *melitensis* infection is rivalled only by that met with after *Spirochæta pallida* infection. I made no diagnosis at the time, and took full notes in the hope that some day I might be able to clear it up. Now I am certain that the liver condition was secondary to Malta fever. The patient was brought into the village from a farm on March 25th, 1908, complaining of intermittent pains in the hepatic region, with jaundice and indigestion. She had been married for fourteen years, had had eight children, the youngest one a year old, and weaned nine months before. The only previous illness was enteric fever when very young. Four years ago she was suddenly seized with severe pain under the right costal arch, which lasted in the severe form for a day. She took to bed and the pain gradually subsided, and she was quite well in about nine days. The attack was not accompanied by any colour in the skin, the conjunctiva, the urine, or the fæces. From then until one year ago she was quite well. One year ago she had a return of the same pain, next day she was jaundiced, and the urine was brown, and the motions pale. During the last year she has had these attacks off and on. They are sudden in onset, accompanied by a feverish feeling, sometimes by vomiting, but never by rigors. The jaundice has been constant for a year. In addition, she has epigastric fullness, especially after meals; the bowels are more inclined to looseness than otherwise.

Present Condition.—Skin dark yellow; conjunctivæ markedly yellow; mucous membranes anæmic; fairly thin. Pulse normal in character and 88 per minute. Tongue clean; a few decayed teeth, but none of them badly so. On inspection of the abdomen, it was found to bulge more on the right side of the umbilicus than on the left, and on palpation this was found to be due to a mass filling up the right half of the abdomen from the costal margin to the level of the iliac crest. It was smooth and only tender to pressure at the upper part. That this was an enlarged liver was easily demonstrated by passing the fingers below the lower margin of it and grasping the edge of it between one's fingers and thumb, just as one can often do that of an enlarged liver. In the same way the notch between the two lobes could be easily traced out. There were no signs of ascites. The patient remained under observation for five weeks, and went home feeling much better, but still somewhat jaundiced. I saw her again about a year afterwards, when she looked the picture of health, and was suckling another child. Her liver was still palpable about two inches below the costal margin. I know of no other possible diagnosis than that the liver condition was due to Malta fever. I have not read of any disease associated with such profound jaundice and such tremendous enlargement of liver, and followed by gradual return to perfect health.

If such a case were to occur in the tropics, where Malta fever is common, as has been proved, and if it were to come under observation during a pyrexial wave with drenching sweats, I can imagine that it could easily be diagnosed as hepatic abscess, and pus looked for.

KIDNEY CASES.

The two following cases occurred in the early days of my Malta fever experience, and gave me considerably greater difficulty in diagnosing them than they would now:—

Case No. 7: Female, aged 18, Shop Assistant.—I was called to see this girl on August 29th, 1908. About a fortnight previously she had had tonsilitis with general pains and malaise, but no rash. She had been in bed for a week, and after getting up she went at once to her work in a store on a cold, raw day. On August 25th her relatives noticed that her eyelids were puffy, and since then the œdema had become general. When I saw her, it was after she had been a day in bed again. Her eyelids were puffy, there was no œdema elsewhere, the tongue was clean, the temperature normal, pulse regular, of high tension, and its rate 80 per minute. Heart signs were normal. Urine was scanty, with specific gravity of 1016, and was loaded with albumen. The deposit contained much epithelial débris and a few hyaline and granular casts. The ophthalmoscopic appearances were normal.

A diuretic was given, and the patient put between blankets, and hot-water bottles applied to promote the action of the skin. For the next three days she had most profuse perspirations and severe headaches. The former I considered to be due to the treatment pursued, and the latter uræmic in nature—both wrongly, as the sequel will show. The œdema disappeared, and she gradually improved. Almost daily examinations of the urine showed that the albumen was gradually diminishing, so that at the end of ten days there was just a cloud. On no subsequent examination of the deposit were tube casts found. The daily amount of water was also registered, and found to vary between twenty-four and forty-eight ounces.

She was kept to the house for another fortnight, improving gradually, when, without permission, she went out for a drive in fairly cold air. Next day her urine was quite "thick," and on the next, *i.e.*, October 1st, it was quite red. I was hurriedly sent for. The urine was as bright red as weak claret, and loaded with albumen. The deposits contained no casts and no red blood corpuscles—that is to say, the condition was one of hæmoglobinuria. She was kept strictly in bed. Her only complaints during the next two or three weeks were intermittent epigastric pains of a crampy nature. On examination, nothing but tenderess on palpation there was made out. The condition of the hæmoglobinuria was for the first few days intermittent, *i.e.*, one day the urine would be very red and the next day only smoky. Indeed, this difference in degree was noticed even in the course of one day. The kidney and bladder regions were thoroughly examined, with negative results, no evidence of tumour being found. There was nothing in the history to suggest calculus.

I came to the conclusion that the illness from which she had suffered a fortnight before I was first called in was "scarlatina sine eruptione," that she had developed nephritis, and that it was one of those rarer cases of Bright's disease in which one gets attacks of hæmaturia now and then. Based upon such a diagnosis, my prognosis was necessarily gloomy, and caused a considerable amount of unnecessary suffering to the relatives, as after events showed.

During one day early in October, when on my round of visits, I happened to visit this patient immediately after having visited a case of acute Malta fever, whose blood gave positive reaction at a dilution of 1 in 1,000. During the usual routine examination I looked at her tongue, and was struck with its great similarity to the tongue which

I had just seen in my last patient, and began to wonder if I had to deal with a condition secondary to Malta fever. I at once took some blood and sent it to Cape Town, where it reacted positively at a dilution of 1 in 700.

The patient made a gradual slow recovery, and went away for a change of air. Since her return she has continued regularly to attend to her duties in the store. For purposes of this paper I examined the patient's urine on September 16th, 1910, and found it to be normal.

Case No. 8: Male, aged 35, Farmer.—This patient had suffered periodically for about four years in the following manner. He would first lose his sense of well-being, and in a few days headaches would supervene, and then pain in the lumbar regions, especially the right. The latter was of a dull character, passing down in the direction of the right ureter into the right testicle, and down the inner side of the right thigh; it was never of a colicky nature, and was never accompanied by any peculiarity in the water, or in the act of micturition. In addition, at these times he would suffer from dyspepsia and flatulent distension. The duration of these symptoms was generally a few weeks, and similarly the intervals of freedom from them.

Complete notes of an examination made on July 31st, 1908, were taken, and the following is a summary of them: Tenderness on gentle palpation in epigastric region. Deep palpation discovered a smooth and tender liver extending one and a half inches below xiphoid cartilage in the middle line and half an inch below the costal margin in the nipple line. Deep palpation in the right renal region discovered a smooth and tender "lump" corresponding in every way to an enlarged kidney. Deep palpation in the left renal region discovered a similar structure, but not so distinctly. The urine was normal. The patient was being severely handicapped in his attention to extensive farming interests by the worry associated with the above recurring symptoms, so on the above date I advised him to go to Port Elizabeth for another opinion. Before he had made arrangements to follow that course, however, the case last narrated suggested to my mind the advisability of having his blood submitted to the Widal test. This was done, and the result was positive at a dilution of 1 in 300. The effect of having a positive diagnosis of his condition had an excellent mental effect and enabled the patient to put up with the symptoms more cheerfully. I think that he had been haunted by a fear of an abdominal "tumour."

I am inclined to the view that his symptoms during these few years were due to *Micrococcus melitensis* infection, and that the kidney enlargement had a similar origin. My experience is that when *Micrococcus melitensis* infection produces very little pyrexia, it produces symptoms which may last off and on for years.

NERVOUS CASES.

Case No. 9.—A Hottentot woman, aged about 60, came to see my brother, during my illness, on account of the condition of the right eye. He found ptosis of the right upper eyelid, dilation of the pupil and external strabismus, *i.e.*, signs of paralysis of the third cranial nerve consequent, evidently, on a neuritis of it. The typical moist, white tongue suggested an examination for other signs of Malta fever.

None were found, and blood was taken which reacted positively. This woman has not had any acute waves.

I have lately had a case in a child of two years where facial paralysis developed. Her blood reacted positively.

Case No. 10: Male, aged 20 years.—This lad was carried into my surgery by his brother late on the evening of April 15th last on account of spasms which had begun about an hour previously. He was laid on the couch, and I found violent clonic spasms of the right quadriceps extensor muscle to the number of two hundred a minute. The head was being also spasmodically jerked to the right. A quarter of a grain of morph. sulph. was injected hypodermically, and the lad carried home and put to bed. He had at intervals through the night several attacks: I was sent for to see him in a particularly severe one. He never lost consciousness, and the facial expression was the picture of nervous distress. The above was the method by which an acute one-wave attack of Malta fever was ushered in. His blood reacted positively at a dilution of 1 in 60. After that first night he had no recurrence of the spasms, although for a few days he had to be kept very quiet in a darkened room because he said that he had a constant feeling in his right inguinal region as if the spasms were going to begin—a feeling which seems comparable to the aura of the epileptic.

Cases Simulating Hip-Joint Disease.

Case No. 11: Female, aged 7.—On April 26th, 1905, I saw what I took to be the first case of tubercular hip-joint disease which I had encountered in this country.

The following are particulars from notes made at the time:—

"Patient stands with right leg rotated inwards and adducted, and with knee flexed; walks with leg in similar position and with a lordosis. Lying on back, the left leg is 1⅜ inches longer than the right. Lateral movement of right leg moves whole pelvis along with it, while left leg can be moved from side to side with pelvis stationary; right knee cannot be flexed on chest; extending right leg flat on couch produces a lordosis; right leg somewhat wasted; right gluteal muscles flattened; knee-jerks exaggerated on both sides."

The child was taken out to the farm on the same day, and no special treatment pursued. I met her some months afterwards, and was astonished to find her gait and hip-joint normal in every respect, and she has remained well ever since. The case remained a mystery to me until I had made a closer acquaintance with Malta fever and its effects.

That I have erred in good company is evident from the following case:—

Case No. 12: Female, aged 3.—During 1908 the mother of this blue-eyed, fair-haired and pink-and-white complexioned child was under my care suffering from incipient phthisis pulmonalis. At her visit to see me, on December 24th, she mentioned that the child was complaining of pain in the left hip-joint. A rather cursory examination discovered nothing to account for it. During January I was told by an uncle of the child that she could scarcely walk, and limped about with the leg flexed at the knee. During February the mother went to the coast to have another opinion about her lungs, and at the same time the child was taken to a gentleman who had special qualifications in surgery. He diagnosed commencing tubercular disease of the hip-joint, and kept the child under his personal treatment by lying in bed with extension applied to the leg. I got these latter particulars from the parents on March 17th, 1909, *i.e.*, about ten days after they had returned from the coast. I happened to be on the farm that day, and took the opportunity of making a thorough examination of the child. I found all the movements of both hip-joints to be perfectly normal, the legs to be equal in length, and the gait to be normal The liver, however, was palpable two inches below the right costal margin, and the spleen half an inch below the left. I took blood which reacted positively at a dilution of 1 in 150.

The child has remained well, although the mother unfortunately died of the phthisis pulmonalis in the early part of the year.

August 29th, 1910.—On this date, and while this paper was being prepared, this child was brought to me with the history that for three weeks she had been off her food and her games, and had been sleeping restlessly. For two days the water was noticed to have been very dark. She was very pale, but there was no œdema. The tongue was coated with a dry, yellowish-white fur. The spleen was not enlarged, but the edge of the liver was felt about an inch below the costal margin. The urine was blood-coloured and loaded with albumen.

September 6th, 1910.—Urine examined to-day was found to be of a dirty yellow colour, with a specific gravity of 1007. It had a distinct precipitate of albumen, and the deposit showed granular and blood casts in fair abundance, as well as renal epithelial cells, leucocytes, and red blood-corpuscles.

It is evident that the *Micrococci melitensis* in this case have been lying dormant during the interval of good health.

The renal condition would appear to be very similar to that which existed in Case No. 7.

Injuries, Operations, and Confinements in Patients Suffering from Chronic Malta Fever.

Case No. 13.—On April 10th last a little coloured boy, aged about 8 years, was brought into the village suffering from a compound fracture of the left femur and severe laceration of the left foot, the result of a wagon accident.

After he had been under care for a few days the temperature began to rise. There was nothing discoverable at the sites of the injuries to account for this, but a general examination revealed signs of Malta fever, and his blood reacted positively at a dilution of 1 in 35. That discovery relieved one's fears of septic infection of the compound fracture. By the time that the latter had healed sufficiently to permit of the lad leaving his bed the pyrexial wave of the Malta fever had also subsided.

Case No. 14.—On January 14th, 1910, a huge tumour of some seven years' standing was removed from a man aged 40. Everything went well with the patient and the wound for ten days, when a subacute attack of Malta fever with pleural effusion developed.

This patient died suddenly some weeks after returning to the farm. I had no opportunity of ascertaining the cause.

Dr. Strachan, in his latest paper on Malta fever, mentions that when this fever complicates the puerperal state grave apprehensions of septicæmia are apt to arise. I remember having such a case in the days when I was unacquainted with Malta fever. It gave much anxiety, and puzzled me not a little.

In the above three cases I take it that each patient was suffering from chronic infection with the active symptoms and signs held in check. In the one case the injury, in the other the operation, and in the third the confinement, upset in some way the equilibrium between attack and defence which had been maintained until then.

The above cases will suffice, I hope, to demonstrate that the South African practitioner must be prepared to meet with *Micrococcus melitensis* infection under extraordinary disguises. Other illustrative cases, the symptoms and signs of which centred around other organs, might have also been given.

DURATION.

There are two points connected with this question, namely, the pyrexial duration and the duration of the period of ill-health.

Hughes give the average pyrexial duration of 372 cases as 58 days. My impression is that the average pyrexial duration of South African cases is much less. On the other hand, subacute and chronic cases may take years to get rid of the effects of the disease.

PROGNOSIS.

Hughes puts the case mortality as two per cent. in the Mediterranean. It is even less in South African practice, where one has more subacute and chronic cases than would be met with in hospital practice in Malta. As far, therefore, as mortality is concerned the prognosis is favourable. It is quite otherwise, however, when one comes to consider the question of the after-effects. Some of them constitute a serious menace to life itself. That, however, is fortunately exceptional. A serious aspect of the prognosis of the after-effects is the great length of time that some of the cases take to regain their former state of health, and the tendency in the subacute and chronic cases to have apyrexial relapses of some of their symptoms, not sufficiently severe, perhaps, to incapacitate the patient from work, but bad enough to interfere greatly with his or her comfort.

Some of the cases described in the paper show that in many apparently serious conditions when it has been established that the diseased condition is secondary to *Micrococcus melitensis* infection the prognosis becomes improved thereby.

Generally speaking, the more sudden the onset and the more acute the pyrexia the shorter is the duration, and the greater the freedom from after effects.

RECURRENCES.

These must be discriminated from relapses, but how that is to be done appears to me to be rather difficult. An attack brought on by a return to activity of micrococci which have lain dormant somewhere in the system, for years perhaps, must be called a relapse rather that a recurrence. While Eyre states that records of authenticated second attacks are very rare, he cites his own case as proof that they do occur. There was an interval of two and a half years of perfect health between his attacks. The second attack followed on taking part in the cleansing of a laboratory in the dust of which *Micrococci melitensis* were found.

When in London last year, I heard of a case of a second and fatal attack, in a lady, which occurred in Ireland ten years after she had left Malta, where she had had her first attack. This must have been due to a return to activity of micrococci which had remained dormant somewhere in the system of the person during all these years, and analogous to what we get in tubercular infection.

SEQUELÆ.

This part of the subject is so very important to us as South African practitioners that I shall not attempt to deal with it in a small paragraph at the end of this paper.

IV.—HYSTERIA.

By R. D. Parker, M.A., M.D. (Cantab.), M.R.C.S. (Eng.), Caledon.

(*Abstract.*)

Hysteria is not a disease of over-civilisation, although nervous exhaustion often produces similar symptoms. The truly hysterical person is one who has "thrown back" as regards mental development to the condition of the savage, or who in adult life remains a child.

Among hysterical people, children and savages—(1) prolonged attention to matters not of personal interest is impossible to maintain: (2) they are thoughtlessly cruel and revengeful; (3) are prone to theft, not realising its criminal nature; (4) are inveterate and purposeless liars; (5) their idea of humour is closely allied to cruelty; (6) the unseen world is intensely vivid and real to them; (7) they are subject to convulsions from trivial disturbances; (8) they are very amenable to suggestion.

Civilisation tends to altruism, children and savages are *egoists*, and the same is true in an exaggerated degree of the hysterical. They are natural *mystics*, unable to think clearly or distinguish truth from falsehood. An infant is hyperconscious of internal processes, and less conscious than an adult of external stimuli. In the normal individual continuous bodily processes, so long as they are healthy, do not invade consciousness; in the hysterical any bodily process, such as digestion or cardiac action, is apt to invade consciousness. Even two or three of these processes are apt to cause discomfort at the same time. Which group of symptoms shall be most prominent is largely dependent upon the suggestion of the medical man. Practically it is almost impossible to render *all* unconscious, although the "Christian Scientist" apparently sometimes succeeds even in this. It is best, both for the friends of the patient and for the medical adviser, to render the gastro-intestinal

symptoms prominent. The actual working of the organs from which the patient's attention is distracted improves. Sick people with one ailment add "hysterical" symptoms. Taking only *muscular sense* as an example, we get paresis of comparatively healthy nerves in locomotor ataxia and in rheumatism; dyspeptic patients may feel their heads swollen, the convalescent enteric feels his body heavy. Every sick person becomes more or less hysterical, and cannot be treated as a normal individual.

The treatment of hysteria is mainly by suggestion, but patients' true characters must be realised. They may appear more or less intellectual; really they are stupid. Words have but little effect. To make an impression a suggestion must be very emphatic and frequent. In some the "seat of the mind" appears to be the lumbar enlargement of the spinal cord, and their ideas revolve round the action of their generative organ; in others it is the solar plexus. Their brains to external impressions are dull.

Their sufferings can largely be traced to nerve energy taking wrong paths, or acting in a disordered way. Thus, we may have general convulsions, or muscular fibres may be affected, causing spasmodic stricture of the œsophagus, hysterical vomiting, spasmodic stricture of the intestine, chronic constipation, or even intestinal obstruction, spasmodic dysmenorrhœa, or retention of urine. Babies sneeze when brought into bright sunlight; many patients swallow when you put drops into their eyes. Krafft-Ebing records a case when sneezing took the place of the sexual orgasm. I had a patient who explained that the sight of a pretty girl often caused him violent attacks of sneezing. These discharges of nerve energy also effect vaso-motor nerves, and hence the nutrition of various organs.

Can we by an idea or suggestion affect these vaso-motor nerves? I think we can. In the case of erectile tissue the effect of an idea is obvious. The idea of food stimulates the salivary and gastric glands. The suggestion that you will cure a lumbago or sciatica, re-inforced by a few punctures in the neighbourhood affected, is frequently successful. Kidney pain and hæmaturia have been frequently cured by operation when no disease of the kidney was discovered. Some surgeons have obtained remarkable results by removing the appendix for dyspeptic symptoms. Even with normal women much may be done by suggestion in controlling labour pains. It is a common idea with midwives that the coming of the doctor frightens away the pains. The fact of the matter is that the mere advent of the doctor has a sedative effect. At first the pains vanish; soon they return more powerfully and more effectively. If, however, they are still rapid and ineffective, with the hand on the uterus the medical man can frequently get a little ahead of the patient in suggesting that the pain is passing off and that she should rest. Having gained influence, he can proceed to suggest that the smaller pains are not *real* pains, and that she should not bear down with them. Such pain will usually rapidly pass off. When he feels a stronger pain coming he can suggest that this is a real pain, and that she should utilise it to the utmost. Not only the voluntary muscles of the abdomen but the involuntary muscles of the uterus will rally to your suggestion. This you can all verify for yourselves.

In checking vomiting, too, the mere prescription of medicine is commonly useless, but if you will remain with your patients and, remembering that the assumption of an attitude suggests an idea and even an act, insist upon their lying flat, and at the same time assure them that with the medicine you have administered their vomiting will cease, you will probably succeed. I believe that even more may be done. Lombroso, when not writing at all of the therapeutical value of suggestion but of hysteria among criminals, records a case of a woman who, when charged with theft, was thrown into violent hysterical convulsions combined with uterine hæmorrhage. Ordinary treatment having failed, he hypnotised the patient and suggested that the hæmorrhage should stop, which it did "as if by magic." It subsequently recurred, and the same treatment was equally effective. That the nervous tracts conveying painful impressions can be shut off by suggestion is illustrated by the well-known case of Harriet Martineau, a professed atheist and a strong-minded woman, who, after long disablement, was cured by mesmerism of a pelvic tumour. I have been unable to get full details, but as far as she was concerned she was cured, although the tumour was found *post-mortem.*

The evidence of the cures at Lourdes, although not perhaps strong enough to convince medical men with a strong prejudice against them, is strong enough to hang a man three or four times over, and these cures are by no means confined to nervous ailments.

Successful medical men usually employ suggestion, but not systematically and consciously. Many refuse its aid as savouring of charlatanism, but first I would say to them, we *cannot afford* to neglect it.

More people (according to Professor Osler) frequent Lourdes than all the hospitals of France, and the same is true of the shrine of St. Anne de Baupré in Canada. Christian Science is growing and certainly effects "cures." In Germany the "Nature Healers," masseurs and "electric healers" already outnumber the regular medical practitioners by three to one. No sensible person doubts the "suggestive" factor in the cures of the hundreds of thousands of people who visit mineral water springs. In 1907, 3,946 individuals graduated in the United States as osteopaths. Many clergymen are now advocating "the restoration to the Church of the Scriptural practice of Divine healing."

But much more strongly I would say, it is a real power, the neglect of which is causing the medical practice of the world to drift into the hands of charlatans. Leonard Williams states: "It is no longer possible to doubt the results themselves; anyone who takes the trouble to investigate the matter with an open mind will very soon be convinced of their reality."

The proper use of suggestion, and it must be repeated suggestion, when employed simply for the patients' good, by men who are qualified to know the limits of possible results, should rank as equally scientific and no less reputable than the administration of drugs. It requires infinitely more patience, time, and energy. If we do not consider it good that others should usurp our functions we must again become the High Priests of the Healing Art.

No patient should ever know what he is taking. The layman's familiarity with drugs has practically done away with the value of suggestion in the taking of medicine. The swallowing of each dose of medicine should be a form of "absent treatment." The value of the spiritual part is a difficult subject. I personally lay great stress on it.

I would only say:

> "The world is too much with us, late and soon;
> Getting and spending, we lay waste our powers,"

and also quote Clifford Allbutt: there is "a health not only of the belly, not only of the heart and common brain, but also of the rarest and most exquisite textures of the cerebral web. If in rude health of the grosser body these subtlest parts have not been exercised and cherished, the total harmony is diminished." The first symptoms of insanity are usually deterioration of *character*. It is character that is first affected by morphia or alcohol. This part applies more to patients drifting into neurasthenia than to true hysterical patients. Mrs. Eddy's success is easy to understand. She provides a mystical religion for natural mystics. She repeats her ideas so that they must reach the dullest brain. That all suffering is an "error of the mind" is for the hysterical more easy of belief and more comforting for their self-esteem than the belief that only their particular sufferings are so. Cannot we learn something from her in the legitimate practice of our art? I can only suggest a little more mystery, a little more appeal to the imagination, such modification of methods of dealing with hysterical patients as a better knowledge of their characters may suggest; and for the larger class of really normal people who are drifting towards neurasthenia from the fret and stress of modern life some attempt at a treatment which I have endeavoured to suggest by my quotations.

As regards the more practical "Weir-Mitchell" treatment, I have often seen utterly unsuitable patients sent to hospital for its administration; patients whose minds are already blank enough without being kept isolated, who shew no sign of exhaustion and are often even fat. The test of successful Weir-Mitchell treatment is usually the gain in weight; for fat patients a preliminary starvation until 14 or 20 lbs. has been lost, and then the regular treatment has been recommended (Allbutt's *System of Medicine*). This appears to me rather nonsensical. The sick-room is unsuitable for them. The success or failure of the treatment depends almost entirely on the personalities of the medical man and nurse. The medical man must make a daily visit, and say the same thing with ridiculous emphasis over and over again. It is a case of treatment by suggestion, aided by the removal of a possibly even more obnoxious previous environment.

V.—IONIC MEDICATION BY ELECTROLYSIS.

By H. W. Reynolds, M.B. (Lond.), Sea Point.

An electric current has the power of decomposing medicinal and other substances into their constituent parts, the products of decomposition being in a nascent or "ionic" state. In this state, as is well known, chemical substances are in a much more active state than normally, and therefore medicinal substances coming into contact with the tissues in an ionic state have an enhanced effect.

Now it has been proved that mere solution of a substance in water converts part of it into ions, but the electric current has two further effects: firstly, it causes the ions to penetrate the unbroken skin; and secondly, it causes a repulsion of basic ions from the positive pole and an attraction to the negative pole, whereas acid ions are repelled from the negative and attracted to the positive pole. Consequently, if lint soaked in a solution of, let us say, zinc chloride be placed in contact with the skin and covered with an electrode connected with the positive pole of a battery, zinc ions will be driven in through the skin, the chlorine collecting in the lint. If the negative pole were used, chlorine ions would be driven in and zinc would collect in the lint. As to how deep into the tissues the ions penetrate we do not know with certainty, but judging by therapeutic results, the depth must be considerable in some cases.

The other pole of the battery must be placed at some other part of the body, and is called the indifferent pole, because we do not desire any therapeutic action there, though a similar process is going on. For instance, if we have zinc chloride at the positive pole and Na_2CO_3 at the negative, zinc is driven in from the former, as we saw, and CO_2 from the latter, but we do not look for any therapeutic result from the CO_2.

I had hoped to demonstrate the penetrating power of the ions by a little experiment, but difficulties of current supply have prevented this. I may just describe briefly what I had intended. A number of filter papers soaked in starch solution are alternated with layers of parchment paper, a rubber diaphragm being put between each to concentrate the area of current. Thus we have parchment paper, rubber diaphragm, filter paper, parchment paper, rubber, and so on for several layers. On top of the top layer is put a pad of lint soaked in 2 per cent. KI, and over that a flat electrode of carbon connected with the negative pole; at the bottom is a similar electrode connected with the positive pole. On passing the current the ionic iodine from the top pad of lint passes through the parchment paper (representing the skin) and turns the filter papers blue, the depth penetrated depending on the strength of current. This shows both the decomposition of KI and the penetration of the iodine ions.

It may be asked at this stage whether it would not be much simpler, and just as effective, to introduce the drug into the tissues by hypodermic injection. The answer is,—No, for apart from the fact that it is in a far more active state, the drug penetrates into the protoplasm of the cells, whereas a hypodermic injection merely sends it into the interstices, and thence into the bloodstream, where it is lost.

The first consideration is the electric current and its source. The current must be a continuous one of about 25 to 50 volts, with an arrangement for graduating the voltage from zero up to the maximum. The best source is a main town supply, and I shall describe this first, as it is the form I have worked with. A continuous current of 110 volts is connected with a lamp and sliding wire resistance arranged so that when the slider is to the left the voltage is zero, and as it is pushed to the right it increases up to 26 volts. Included in the circuit is a milliammeter, measuring the current actually passing through the patient in milliampères. The terminals are marked + and —. Attracted to this are flexible wires to which electrodes of various kinds can be connected. I may say here that if the only main current available is an alternating one it must be transformed either by some pattern of transformer, a rather expensive piece of apparatus, or else an accumulator must be charged from the main, and used as the source of current. If no main current is available one must rely on a battery of twenty or thirty cells.

The choice of electrodes is a matter of great importance. Flat carbon plates are useful in many cases, and these can either be placed over a thick pad of lint or wool and bound on to the part to be treated or they can be immersed in a solution of the drug to be used, contained in a bath which can receive a limb. Zinc or copper electrodes may be used over pads soaked in zinc and copper solutions respectively The indifferent electrode may be a large flat plate of modelling clay zince or carbon, or a carbon plate dipping into a bath filled with say, bicarbonate of soda.

If a metal or basic radicle, such as zinc, copper, lithium, magnesium, or cocaine is to be driven in it will be done from the positive pole. An acid radicle such as salicylic acid or iodine will be driven in from the negative pole.

The chief point to consider in regard to the indifferent pole is the avoidance of burns. These can be avoided (1) by having a large electrode, thus reducing the current density per sq. cm; (2) by having a number of layers of lint soaked in your solution, or having a limb immersed in a bath of the solution, either of these ways tending to dilute any caustic ions given off; (3) by the use of a solution which does not give off very caustic ions, *e.g.*, lithium at the positive pole is less caustic than sodium evolved from saline solution, and carbonic acid evolved from sodium bicarbonate at the negative pole is less caustic than chlorine from saline solution.

To illustrate these points, as well as in continuation of the subject, I will now describe the treatment of some cases which I have carried out. I do not desire to weary you with all the diseases that may be treated by ionisation, so I will enumerate those which I have treated:—(1) Chronic eczema, by zinc ions; (2) ditto, by lead ions; (3) warts, by magnesium ions; (4) carbuncle, by zinc ions; (5) ditto, by salicylic ions; (6) rheumatoid arthritis, by iodine ions; (7) removal of sebaceous cyst after introduction of cocaine ions.

(1) Mrs. G. had a patch of very dry and chronic eczema in front of each knee for three years. She had been treated by several medical men without benefit. I myself treated her for six months with lotions and ointments of various kinds, but the patches remained the same. I then began the treatment with zinc ions as follows:—Fifteen to twenty layers of lint were soaked in a 2 per cent. solution of zinc sulphate and laid over one of the patches. This was covered by a zinc electrode about 2 inches × 1 inch and connected with the positive pole. The negative pole was larger, about 6 inches × 3 inches, and applied sometimes to the same leg lower down, sometimes to the opposite leg. A current as strong as the patient could stand was passed for thirty or forty minutes and repeated twice a week. This patient was rather sensitive, and could not stand more than eight or ten milliampères. After sixteen applications the improvement was not very marked, so I decided to employ lead ions, remembering the value of lead salts in many eczematous affections. The same procedure as before was followed, a 2 per cent. dilution of liq. plumbi subacetatis being substituted for the zinc sulphate, and a lead electrode for the one of zinc. Fourteen applications brought about a cure, though, as sometimes both patches were treated at one sitting, and sometimes only one, each patch had about ten applications. I am unable to find any record of lead ions having

been previously used either for eczema or any other disease, and I am unable to say whether the first part of the treatment (*i.e.*, the zinc ions) had any real effect, but there was no doubt as to the efficacy of the lead.

Three other cases of eczema have been treated with zinc ions. They were acute cases which had hung fire, so to speak, *i.e.*, had ceased to improve under ordinary treatment. They were all cured after a varying number of applications, but as other treatment was continued throughout, it was a little difficult to estimate the exact amount of benefit to be attributed to the treatment by ions. However, the impression left on one's mind was that improvement, which had ceased previous to ionisation, began again and lasted until a cure was effected.

(2) I will next mention a case of multiple warts treated by magnesium ions. This treatment was originally suggested by the fact that some observers had reported good results in cases of warts by the internal administration of magnesium salts.

D., a little boy of about 7, had a large number of ordinary warts, I think fifty or sixty altogether, on the dorsal surfaces of both hands, extending from the wrists to the nails.

About half the affected surface of one hand was covered with several layers of lint soaked in a 2 per cent. solution of magnesium sulphate, and as strong a current as the patient would stand was passed—it was about 10-12 milliampères. He had thirty or forty minutes on twenty occasions, which would mean the whole area was treated five to eight times. After the first four or five applications many of the warts were distinctly smaller, and a few had disappeared. At the end of the above-mentioned twenty séances the improvement seemed to have ceased and, as it was rather trying to so young a patient, I did not care to go on. To my surprise the child was brought to me two months later absolutely clear. This seems very interesting, as it suggests the question whether the magnesium had been stored up in some way in the cells—at any rate in some way the effect seemed to have been continued. No other treatment had been used, and I hardly think that nature would have produced this result unaided.

The treatment of carbuncle (or even boil) by zinc ions is very valuable, for we all know what an extremely painful and lengthy business it is. If the carbuncle is already open a zinc needle may be pushed through the opening at the positive electrode—otherwise a pad soaked in 2 per cent. zinc sulphate is placed over the area as for eczema. Thirty milliampères can easily be borne when the wound is open, perhaps only 15 otherwise. In either case the relief of pain is rapid, and the patient will tell you next day that he had his first good night for a week. Three, four, or perhaps five séances will produce a healthy, painless wound. Thirty to sixty minutes may be given according to the strength of the current. Recurrent boils of the nape of the neck may be similarly treated with success.

I have not yet tried the treatment for comedones or acne, but this is usually quite successful, though weak currents are necessary about the face—salicylic ions are recommended for these conditions, the negative pole of course being used; 5 milliampères for about ten minutes two or three times a week till cured, and then every ten days or so for a few months to prevent recurrence.

Salicylic ions may also be used in the treatment of carbuncle, and the fact that these ions seem to have a special value in the relief of pain leads me to speak of their use primarily for this purpose, whether in inflamed wounds or in neuralgia, for instance. Indeed, one might say that any inflamed, painful or non-healing wound treated in this way will be benefited, pain vanishing, inflammation subsiding, and healing commencing after one or perhaps two applications. Fifteen or twenty milliampères for forty minutes daily will be advantageous here. In neuralgia the same treatment may be applied to the seat of the pain, with very gratifying results.

We now come to the treatment by iodine ions of osteo-arthritis, chronic rheumatism, and thickenings round joints as a result of injuries. These are very intractable cases to treat by ordinary methods, and it is very gratifying to have a method at one's disposal that really produces tangible results. I have worked here with baths in which the limbs can be immersed, and a case at present under treatment is dealt with in the following way: The left elbow, wrist, and joints of hands are affected with osteo-arthritis, and so the whole forearm is immersed in an arm bath containing a 2 per cent. solution of potassium iodide, which has enough tinc. iodi. added to it to make it of a sherry colour. A carbon electrode connected with the negative pole dips into the solution. Some workers advise confining the current to one side of the body, to avoid having the spinal cord in the circuit, but to avoid undressing I immerse the other arm in a bath of carbonate of soda solution, into which the positive carbon electrode dips. A lithium salt would be less corrosive, but I have not found burns resulting from the dilute soda solution, so keep to that for the present. A current of 20 to 30 milliampères is passed for about forty minutes two or three times a week. This patient has no pain, but the movements are getting less limited, and I am encouraged to persevere, although the disease is of too long standing to hope for complete cure. In other cases where pain has been prominent the relief has been more marked, and thickenings following healed fractures have been quite eliminated.

In all these cases, potassium iodide is administered by the mouth throughout the treatment, as, in the words of one observer, "any iodine compounds lying in or circulating through the joint tissues—should the patient have been taking them for some time and in fair quantities—are broken up by the current and locked up in the joint tissues temporarily, instead of being carried through the blood vessels and excreted." The same observer advises treatment by radiant dry heat at 400° F. previous to each séance, but I have been unable to carry this out for want of apparatus.

The use of cocaine ions for producing local analgesia is simple. merely the method previously described, the usual pad of lint being soaked in a 2 per cent. solution of cocaine hydrochloride and covered by a carbon electrode connected with the positive pole; five or ten milliampères suffice to produce perfect analgesia in five or ten minutes. I have had a small sebaceous cyst removed from my own neck in this way, and can testify to its painlessness. Operations on small tumours or varicose veins, extraction of teeth and other minor procedures may all be dealt with thus. It has advantages over infiltration analgesia, and really does not take much longer if the apparatus is to hand.

In conclusion, I submit that in treatment by ions we have a very valuable addition to our armamentarium. The difficulties in obtaining the current may be great in country practice, and in any case it takes time and patience, but, applied to cases that we should otherwise be tempted to dismiss as incurable, one has earned the gratitude of many a patient, not to speak of the satisfaction that victory over disease brings. The treatment is in its infancy, and probably by the time we assemble again in Congress much new ground will have been gained. I have made no effort to collect in this paper all that has been done by others, *e.g.*, I have not touched on the treatment of rodent ulcer by zinc ions or that of ringworm by copper ions, but have merely tried to give my own experiences, which I hope will be added to by others.

VI.—THE LEPERS OF SOUTH AFRICA.

By S. P. Impey, M.D., Cape Town.

(*Abstract.*)

Leprosy has exercised the minds of medical men and legislators from time immemorial. With an improved knowledge of disease we have come to the conclusion that it is by *prevention,* not by cure, that we can hope to stamp it out, and it is to this aspect of the leprosy question that I wish to draw attention.

Hansen's discovery of the leprosy bacillus placed the disease on a somewhat sure footing, but the bacillus does not conform to Koch's canons; though we believe it is the cause of leprosy we cannot prove it. It has not been cultivated artificially. Professor Koch told me he had tried every possible medium but had always failed; Dr. Arning claims to have cultivated it, but I saw his specimens and was unable to agree with him, and in any case he was unable to grow sub-cultures; Professor Campana also claims to have obtained a growth by inserting a portion of a nodule in a cock's comb and from this making stab cultures in jelly, but I have also seen his specimens and cannot agree that they shewed leprosy bacilli—a view in which Dr. Edington, to whom I submitted a tube of the cultures, concurred. The organism has never been found alive outside the human body. So far as I am aware, no case of successful inoculation has been reported. Many observers have inoculated mice and guinea-pigs, but always with negative results; some have inoculated themselves, in the interests of science, without result. Arning claims to have successfully inoculated a convict, but it was afterwards discovered that the convict belonged to a leprous family, and that some of his near relations had died of leprosy.

Of the three forms of the disease—tubercular, anæsthetic and mixed—the first is a very severe disease and few recover from it; the anæsthetic form is much milder and many cases become self-cured. Some believe that the bacillus enters the system through the nasal mucosa, but its exact mode of entrance has not been ascertained.

In the tubercular form the exposed or fully ventilated parts—the face, hands and feet—are chiefly affected; in those who habitually wear boots the feet are little if at all affected. The skin nodules teem with the bacilli, and so do discharges from ulcers which form on them, owing to interference with the circulation by the great multiplication of the bacilli. There is one form—rare in South Africa but fairly common in Europe, and always very chronic—in which nodules occur on the trunk, and the skin becomes hard and dry.

In anæsthetic leprosy in my opinion only the nerves are affected by Hansen's bacillus; all other symptoms are of a trophic nature. There are no bacilli in the discharges from sores; Hansen and, I believe, a few other observers say they have found bacilli in the anæsthetic patches, but my experience does not agree with theirs, though the presence or absence of a few bacilli in the skin is not of vital importance; when bacilli are found in the erythematous patches the case is, I consider, one of tubercular leprosy. Some think the anæsthetic skin patches are due to bacilli invading the skin; but this is not so; moreover it is inconceivable that a few bacilli could redden such large surfaces. These patches are due to lesions in the nerves, and their extent is in exact proportion to the severity of the nerve lesions. I am of opinion that except by accident no bacilli are ever found in the wounds in anæsthetic leprosy; it is possible that they may be so found in a case in a leper ward crowded with tubercular cases, but they are then of extraneous origin. I have never found one bacillus in the wounds of anæsthetic leprosy, and in this result I am supported by the observations of many leading leprologists.

In all cases of anæsthetic leprosy it is by no means easy to find bacilli, even in the nerves; some, indeed, believe that leprosy bacilli are never found in anæsthetic cases. If the disease be spread by contagion—which we all, I think, must believe—it cannot be from anæsthetic cases. From the point of view of contagiousness there is as much difference between tubercular and anæsthetic leprosy as there is between phthisis and lupus, and still we go on treating all forms of leprosy as though they were equally dangerous to the public health. Although tubercular leprosy may be theoretically contagious, this cannot be said of the anæsthetic form which, from its nature, must be harmless. In this opinion I am supported by most of the leprologists whom I have consulted. Ehlers and Sand think that, in general, anæsthetic leprosy is not a danger to the public, and that all anæsthetic lepers might be discharged from hospital. Borthen sees no necessity for keeping anæsthetic lepers in hospital. Christie thinks it unnecessary to segregate anæsthetic lepers. Koppel and Dormitz think anæsthetic leprosy is harmless. Koch and Koppel say the discharges of anæsthetic lepers are free from bacilli, therefore the patients must be harmless. Munro thinks anæsthetic cases are not a danger to the public Thin says anæsthetic cases are practically harmless. Jonathan Hutchinson thinks anæsthetic leprosy not dangerous. Lahmeyer does not believe that anæsthetic leprosy is contagious. Kaurin is not prepared to say that anæsthetic leprosy is harmless, because we do not know how contagion occurs, but it certainly is not as dangerous as the tubercular form. Hansen thinks it is only dangerous during the erythematous stage, and even then much less so than the tubercular form; and further, he says, we all agree that when there is no opportunity of isolating all cases of leprosy we would do well to isolate the tubercular cases and leave the anæsthetic free. Dr. Looft is not prepared to say anæsthetic leprosy is not contagious, but the risk attending their discharge is so infinitesimal as to be hardly worth considering. Saltzman doubts if there is any danger in anæsthetic cases. Oscar Petersen says that at any rate during the non-ulcerative stages there is no danger in anæsthetic leprosy. Bergmann is not prepared to say that anæsthetic leprosy is absolutely free from danger, but he has never found bacilli in the discharges from their wounds. Reissner has not found bacilli in the discharges from the wounds of anæsthetic leprosy, and therefore considers they cannot be of danger. Phineas Abraham thinks there is little danger of infection in any form of leprosy. Munro would not recommend the discharge of anæsthetic lepers, not because they are a danger, but because if the disease is not stamped out as rapidly as was expected the originator of the scheme might be blamed for making a gap in the law. Engel Bey says anæsthetic cases cannot transmit the bacilli, but he is not prepared to admit that Hansen's bacillus is the same as leprosy.

From the foregoing it will be seen that nearly all leprologists agree that the danger of liberating anæsthetic cases is hardly worth considering. The tubercular form preponderates in cold, and the anæsthetic in hot, climates. In South Africa to-day there cannot be less than three thousand cases known, and of these about two thousand are anæsthetic. Dr. Gregory acknowledged, in 1906, that the Act was not doing what was expected of it, for want of asylum accommodation, and that in Cape Colony alone there were about one thousand lepers still at large. If the anæsthetic cases were not segregated there would be ample room in the asylums for all cases which might possible be a danger to the public.

In 1892 I first drew attention to self-cured cases of leprosy. The disease does not yield readily to treatment, but, like all other diseases due to bacilli, it tends to run its course; I believe most, at any rate of the anæsthetic cases, would become self-cured if the patient did not die of intercurrent affections, and that anæsthetic leprosy becomes cured much oftener and much sooner than is generally acknowledged. Many say it is difficult to ascertain when cure actually takes place. Hansen says he has made *post mortems* of many such cases and, when he found no bacilli, came to the conclusion that the patients had probably been free from the disease for a very long time; and this has been my experience also. I have made a careful study of hundreds of cases of anæsthetic leprosy and have come to the conclusion that the disease has run its course in about four years from its outset, and that, though thereafter the patients may be much marked and mutilated, they are suffering not from leprosy, but from nerve destruction. In about four years' time the inflammatory process set up by Hansen's bacillus has generally reduced the affected nerves to filaments of fibrous tissue, and in the general contraction and degeneration the bacilli have been destroyed. Ehlers thinks about five per cent. of anæsthetic cases become self-cured. Sand and Koppel showed me several patients in which they said the disease had been cured. Kaurin says cure is the natural result of leprosy. Hansen says anæsthetic leprosy is often cured, in fact he believes that most of the anæsthetic cases become cured. Lie believes in the self-cure of leprosy, and is preparing a work on the subject. De Susa Martin, Looft, Christie and Munro say anæsthetic leprosy is often self-cured. Arning, Thin, Sederholm, Boeck, Munox and Smirnnoff consider that anæsthetic leprosy occasionally becomes cured. Lanmeyer and Engel Bey have seen many cured cases. Oscar Petersen does not think that leprosy is ever cured any more than syphilis is cured. Bergmann is not prepared to say that it is cured, but it is permanently arrested. Phineas Abraham and Borthen say the disease often becomes apparently cured, but breaks out again in after years. Goldsmidt and Reissner do not believe that the disease is ever permanently cured.

I think no one will dispute the fact that self-cure does occur; and if so, is it right that such cases should be kept in an asylum and exposed to re-infection? Recrudescences do occur, but are very rare; personally I have never seen one. One form of the disease is never superimposed on another form, and it is very unlikely that an anæsthetic case will develop tubercular symptoms without a re-infection. A cured case is not able to resist invasion by the bacilli which surround it in a leper ward. This theory, in my opinion, accounts for cases which recrudesce after years of apparent cure.

Segregation of Lepers.—When the Cape Leprosy Repression Act was passed very little was known of the disease; our present knowledge would have led our legislators to frame a totally different Act I advocate the modification of the existing law in the following directions: (1) All cured cases should be discharged at once; (2) uncured anæsthetic cases should be liberated and kept under observation until they become cured, when they should be discharged; (3) pauper anæsthetic cases should be kept in pauper wards, and not among tubercular cases; (4) in respect of tubercular cases a much milder system of segregation should be adopted—somewhat after the Norwegian system, which is wholly effective, yet much less irksome to the patients. Under it—(*a*) All lepers must be isolated, those under the control of and supported by Village Boards, either in hospitals or in separate compartments at home, according to the opinion of the Board of Health. The separation of husband and wife against their wills is to be avoided as far as possible. (*b*) Any leper failing to obey notice to separate from the rest of his family may be compulsorily removed to an asylum. (*c*) Clothing, etc., used by lepers not to be used by others without thorough disinfection. Dr. Holemboe, Inspector-General of Hospitals, informed me that force is never used unless patients disobey all instructions and refuse to carry out the simple regulations laid down. Dr. Sand told me the patients are divisible into three classes: (*a*) The well-to-do, who are isolated at home, where they have to obey instructions, but a man is not forced to live apart from his wife; (*b*) patients who voluntarily enter hospitals, and leave them when they like; and (*c*) ordinary patients, mostly paupers. I was much struck, when in Norway, by the leniency of the regulations, yet leprosy was dying out there; but whether this was due to the isolation system or other causes, I am not prepared to say, for in all the European countries I visited leprosy was decreasing in like manner, though only in Norway and Russia was any system of segregation in operation. Hansen, though he advises isolation, acknowledges that education and improved sanitation have done more to stamp out the disease in Norway than segregation. Leprosy is frequently discussed at Congresses of medical men in Europe, and the almost invariable result is that segregation is advised, but I believe this is done more for sentimental than medical reasons; for segregation is actually carried out only in Norway and Russia, and there only in a mild form.

Leprosy is infinitely less dangerous to the public than phthisis, and yet we spend at least £50,000 a year in dealing with leprosy and let phthisis run rampant—phthisis which is destroying its thousands as compared with the units destroyed by leprosy. This is not reasonable, and I want co-operation in trying to have this anomaly rectified. I do not suggest the abolition of segregation, but I think the law should be so modified as to deal more gently with the unfortunate lepers and, by adopting the simpler form of isolation which is so effective in Norway, reduce the expenditure on leprosy and so provide funds for the suppression of phthisis and other diseases which are doing so much more harm than leprosy.

VII.—OBJECTIVE PARÆSTHESIÆ OF LOCOMOTOR ATAXY.

By Archibald Gow, M.D., Murraysburg.

In this article I will not dwell on such a wide field of work as the paræsthesiæ of locomotor ataxy, but will omit the better known subjective disturbances of sensation, and will give as concisely as possible a synopsis of those data and symptoms relating more especially to the objective disorders of sensation, symptoms of which are only to be elicited after careful examination, but which are present in by far the larger proportion of cases, and which may in some degree tend to increase our knowledge of this important disease.

These sensory paralyses are more marked as the disease advances, being seldom noticed in the pre-ataxic stage, and the consideration of the statistics of cases personally examined by me may be interesting, and when compared with the similar investigations of Charcot at the Saltpêtrière Hospital may be useful. The chief sensations the disorders of which are under consideration are the following—touch, pain, and thermal, consideration also being given to those deeper sensations of joints and muscles, more especially as they are concerned in that complicated sense which distinguishes different articles and which I may be allowed to term the stereognostic sense. The statistics of the more common superficial sensory disturbances are as follows: Out of twelve cases of tabes dorsalis carefully investigated, eleven exhibited symptoms of sensory disturbances, while the twelfth case was one of three months' duration in the pre-ataxic stage, and will, no doubt, run what we consider a typical course and in time exhibit those symptoms in an equally well-marked degree, so that, as a result of those investigations, this fact may be added to our knowledge—that in practically every advanced case of tabes dorsalis areas of anæsthesia, analgesia or disturbance of the thermal sense may be found. In those statistics under consideration the proportions were as follows: Limbs, every case, trunk ten cases of the eleven, and in three instances in the head. Charcot's statistics are equally interesting. Out of twenty cases carefully examined, anæsthesia was found in seventeen, and out of the three others two of them were in the pre-ataxic stage, while the distribution was as follows: In the limbs in every case, trunk in sixteen, and head in thirteen. It is interesting to note from the combined statistics that areas of anæsthesia were found in twenty-seven out of twenty-eight cases of tabes dorsalis in the ataxic stage.

Before considering the individual sensory paralysis, or rather paresis, in detail, it may be mentioned that while all forms may be associated in one case, more especially the anæsthesia and analgesia, the cases where those symptoms are dissociated are fairly common, and then it is found that the analgesia predominates in a marked degree over the anæsthesia, but, curious to state, though analgesia may be a definite symptom it does not always follow that there will be an equally proportionate disturbance to thermal stimuli.

In considering the question of disturbance to tactile sensation, special notice will be taken of its distribution, extent, and degree. While the distribution of anæsthesia is extremely varied in individual cases, in the aggregate it will be found almost constant in certain situations; the soles of the feet, seldom the dorsum; the back and the outer side of the leg, seldom the anterior and inner; a similar distribution in the thigh, gluteal region, also perineum, around the anus—when anæsthesia is found in the latter site incontinence of the bowels prevails, the rectal detrusor being either diminished or lost, indicating also in a marked degree loss of the muscular sense of the sphincter ani. In the trunk, girdle anæsthesia is most common, but the most constantly selective distribution of all is in the upper extremities; while in the lower limb anæsthesia may be distributed over the entire lower extremities and incompatible with the area supplied by the posterior nerve roots, in some degree in the trunk the distribution partakes similarly to that of the spinal nerve roots in broad bands of girdle anæsthesia, but, as stated above, the most constant and selective distribution of all is in the arms, almost typically along the distribution of the first and second dorsal nerve roots as they emerge from the posterior column of the cord, rather than to a lesion or lesions of the peripheral nerves themselves. This more selective situation in the arms may be explained by the progressive nature of the lesion in the cord, and the fact that in those cases where in the upper limbs anæsthesia was found, extensive areas, including in one instance the entire distribution of the lower limbs and trunk, markedly anæsthetic, were also evident, thus affording clinical evidence of the progressive advance of the lesion in the spinal cord. The distribution in the head varies, as elsewhere, from the extreme, enveloping the entire head and face, to patches of anæsthesia over the malar eminences, but in the majority of cases the areas around the eyes and mouth are sensitive to the touch.

The extent of the anæsthesia in the above situations varies considerably in individual cases, and whilst most commonly bilateral, this is not always constant, although, in the case of the upper extremities, where the selective site is so typical, it may be considered that in practically every case the anæsthesia of this area is constant. But in other situations of the body, trunk, head, and lower extremities, patchy areas of anæsthesia are often found unilateral in nature, while in the leg where a sense of constriction is complained of, a girdle of anæsthesia may be found in this situation also unilateral in character.

While in may cases a sensation of numbness is complained of, especially in the soles of the feet, and anæsthetic areas are found in these situations, the disturbance of sensation to tactile stimuli often requires careful examination for its elicitation, as these areas are often found where numbness is not complained of, not so much because it is not present, but because it does not sufficiently impress itself on the sensorium of the patient.

The anæsthesia present may alter considerably in degree, as also in character; while in every case of tabes dorsalis it may be present, yet in many cases it is extremely slight, almost evanescent in character, confined merely to the finest touch of a hair pencil on individual hairs, or to the gentle application of cotton-wool to the skin; yet often the tactile disturbance is much more marked, and anæsthesia even to a comparatively firm touch or pressure is observed, and also in cases where the anæsthesia is not so marked as this the sensation complained of is one more of tingling than of touch.

The anæsthesia is not always confined to the mere absence of tactile sensation, but is often more of the nature of retardation of appreciation of tactile stimuli up to as long as five seconds, this feature being specially marked where analgesia is present. Often in addition, rather than a complete anæsthesia, the loss of sensation is one of degree, the reaction to the touch not being at all in proportion to the stimulus applied; thus a firm touch may be observed as only a light one, and is seen mostly in cases where there is also considerable retardation to sensory impressions, and is often more marked in painful and thermal anæsthesia than of tactile.

In addition to the above abnormalities of sensation to tactile stimuli, there is often a marked loss of the power of localisation of such stimuli, or, as is aptly termed by Horsley in his Linacre Lecture, the loss of the topognostic sense, or atopognosis. This error in the power of localisation may be of a twofold nature, either or both of which being found in one and the same patient; there may be the initial error of the touch being referred to some other part of the body, and in this case the error is of a simple nature confined merely to tactile anæsthesia; but when, in addition to the above, or even in those cases where the patient refers the stimulus to the proper point of application, the patient, quite apart from any muscular incoordination, is unable to place his finger on the point to which the stimulus has been applied or to the point to which he refers the stimulus, the error is a compound one, and of a much more complicated nature, involving as it does, in addition to cutaneous anæsthesia, that of the deeper structures, the muscle and joints.

Analgesia.—The analgesia found in locomotor ataxy bears a great resemblance in its distribution and other aspects to the anæsthesia in the majority of cases, but in cases where these two symptoms are dissociated, the analgesia will be found to be predominant and much more widespread. In fact, in one of the cases on which these observations are based, although the anæsthesia was limited to the soles of the feet, the posterior aspect of the leg, and a small area on the trunk, yet the whole of the body from the waist downward was entirely analgesic, so that it must be admitted that the distribution of analgesia does not necessarily bear any definite relationship to that of the anæsthesia, the loss of tactile or painful sensation being met with either together or separately; this can be the more easily understood if it be granted that the sense of touch and the sense of pain are conducted through different axis cylinders to the sensorium.

Retardation of sensation to painful stimuli is similar to that found when tactile sensation is thus affected, but, if anything, this retardation and, in many cases abnormality of the pain produced, is much more evident with analgesia than with anæsthesia, as can be seen in the following case, where the results of a pinprick were as follows: The patient was entirely unconscious of any sensation of pain until six seconds had elapsed; then there was a distinct sensation of pain, not the acute pain of a pin-prick suddenly commencing and gradually disapearing, rather the sensation of a more continued severe pain lasting for several seconds, while in other cases where retardation of sensation was observed a painful stimulus was not appreciated as such, but only as a touch.

In addition to the above, and often in conjunction, the abnormality of the summation of painful stimuli may be observed, as when a number of pin-pricks successively applied are not appreciated as so many individual sensations, but rather as one continued pain, differing entirely in character from a pin-prick.

The analgesia present in tabes, like the anæsthesia, is not limited to the cutaneous surface, but often involves the deeper structures, as may be seen in those cases where the upper extremities are involved; pressure on the ulnar nerve as it passes round the internal condyle of the humerus fails to elicit any degree of pain whatever, and a similar condition is found in the loss of the extremely acute testicular pain, and also the muscular pain when deep pressure is applied.

Loss of the Thermal Sense.—We will now consider shortly the loss of the temperature sense, which, in tabes, is in no way comparable nor as marked as this important symptom in syringomyelia, being invariably found in the advanced stages of the disease, and only in conjunction with the anæsthesia and analgesia considered above, never dissociated from them, and in distribution always similar; in those investigations under observation, it was a much rarer symptom than the others, being observed twice in the arms, once in the trunk, and once in the legs.

Like the anæsthesia, and in a lesser degree the analgesia, this disturbance of sensation to thermal stimuli does not result in complete loss of sensation, but is only one of degree, the difference in each case where any alteration from the normal was observed never being so marked that the patient did not appreciate the fact that a test-tube of boiling water was uncomfortably hot, but, excluding such instances, the appreciation of differences of temperature was considerably dulled, the successive application of test-tubes containing warm and cold water never being observed as such, but as the sensation of an even temperature, usually the colder of the two test-tubes, sensation to cold being present even when with heat there was considerable alteration from the normal.

Astereognosis.—This interesting condition, loss of the stereognostic sense, or that sene by which the form of objects is recognised, is seldom found in tabes dorsalis, requiring as it does for its observance the loss of practically all different forms of sensory impressions. Stereognosis, far from being a simple sense, requires for its observance the full use of the following sensory impressions—cutaneous, muscle, joint and temperature, and it is only when the combined loss of all is present in the hands that astereognosis is noticed in a lesser degree when the patient, although walking on a hard floor, states that he feels as if he were walking on a soft surface, but in this case cutaneous sensibility only is at fault; a similar statement of the object being of a soft nature, even though the article is of glass, is often made when astereognosis is a symptom. This loss is seldom complete, but is more one of degree; while ordinary domestic objects placed in the hand could be differentiated, but even then could not be named if the difference between them were at all marked; such difference was unappreciable if the objects were at all similar in character.

VIII.—MYLABRIDES.

By W. Watkins-Pitchford, M.D. F.R.C.S. (Eng.), D.P.H.

In October, 1909, a Natal Zulu who described himself as a "herbalist" (*inyanga ya'makambi*) was charged with culpable homicide. The outline of the case is as follows:—

A Zulu, of about 50 years of age, who enjoyed good health but was childless, desired to be a father, and requested the assistance of the accused. The substance prescribed consisted of two heaped snuff-spoonfuls of brownish powder to be taken in *i-twala*. By the advice of the herbalist the deceased endeavoured to induce his wife to share the draught. She refused, and in consequence the husband took the whole. About seven hours afterwards vomiting set in and the patient became very ill. Although greatly desiring to micturate, he could, at first, only pass a small quantity of bloody urine, and soon after there was complete suppression. Vomiting, retching, and attempts to urinate were continued with urgency through the night, and the next day the patient was in a condition of collapse. When asked by a friend what was the matter with him, he replied, "I cannot feel anything." He died about thirty-five hours after taking the medicine.

A post-mortem examination was not undertaken until five days after death, when the District Surgeon, Dr. M. G. Pearson, found the organs in such an advanced state of decomposition that all signs of abnormality had become obscured. From a description of the symptoms given by friends of the deceased, Dr. Pearson suspected that the material which had been administered was one resembling cantharides in its effects, and he suggested an analysis of the drug. The accused admitted that he had given powdered *i-Bungwane,* that is, a tree- or flying-beetle, and he produced some fragments of the insect. By close comparison these fragments were identified with those of *Mylabris transversalis.* This creature is an elongated beetle nearly an inch in length, the black or very dark brown elytra of which are marked, a little in front of their mid-points, by a transverse, chrome-yellow band, whilst near their posterior ends they carry a dull, bronze-red spot. When these insects are dried and ground, the resulting powder is of a brown colour, and differs from cantharides powder in the absence of the familiar glistening, metallic-green particles. By extraction with chloroform and purification with carbon disulphide nearly two per cent. of cantharidin was obtained from the powder.

An ordinary bone kaffir snuff-spoon, when heaped full with the powder, was found to contain nearly five grains; in this instance, therefore, it is probable that about ten grains had been taken. One beetle of average dimensions produced, after thorough drying, four and a half grains of powder.

These beetles are generally admitted by the natives of the district (Amanzitoti) to be poisonous, and they consider that they should be prescribed only by those skilled in medicines.

Enquiry has shown that there are several species of this genus of beetle in South Africa, and that they all contain cantharidin. Perhaps the commonest species in the midlands of Natal is a small beetle with three yellow bands across the elytra; the first of these bands is in the shape of a horseshoe with its concavity directed towards the head; the second and third bands have jagged or wavy margins. The creatures appear to be attracted in specially large numbers by the flowers of beans and peas.

It would be of interest to ascertain from what source the natives on the coast acquired a knowledge of the aphrodisiac properties of Mylabrides. It seems probable that the knowledge came through intercourse with oriental immigrants, for species of the beetles are common in India and China, where they form an article of commerce under the name of "Telini fly."

The Indian and Colonial Addendum to the British Pharmacopœia contains the "Telini fly," under the name of *Mylabris phalerata*, and enumerates several preparations which are practically the same as those of the *Cantharis vesicatoria*. Mitchell Bruce (*Materia Medica and Therapeutics*) states that any species of this beetle may be used pharmacologically, provided it contain as much cantharidin as the *Mylabris phalerata*, *i.e.*, from one to two per cent. The *Cantharis vesicatoria* of the British Pharmacopœia, which is the usual source of our vesicant preparations, contains only .4 to 1 per cent. of cantharidin. We have in South Africa, therefore, what appears to be an abundant supply of cantharidin-yielding beetles. As some of our species contain more of the essential principle than cantharides, they might with advantage be substituted for them in the manufacture of blistering preparations. The possible use of these insects for criminal purposes makes them objects of interest to the medico-legist.

B.—SECTION OF SURGERY.

I.—URINARY STONE: SYMPTOMS, DIAGNOSIS AND TREATMENT; INCIDENCE IN SOUTH AFRICA.

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(*Special Subject for Discussion.*)

We need not enter into the one-time vexed question of the definition of a calculus. So far as the kidney is concerned, it suffices for our present purpose to define the calculus as any concretion found in the kidney which is incapable of being passed into the bladder.

Calculus may originate in the tubules, the calyces or the pelvis of the kidney. It has become customary to speak of cortical or pelvic calculus, according to the position in the kidney in which a calculus is found; but it must be remembered that an originally cortical calculus may become pelvic, or may not infrequently combine both these situations.

Dickinson and Roberts divided calculi into primary and secondary, a useful and practical classification if we bear in mind that the two groups may be, and, in fact, to a greater or less degree usually are, combined. The secondary calculi consist of phosphate of lime and ammonio-magnesian phosphate, and are due to septic decomposition process taking place in the pelvis or calyces of the kidney. The primary calculi, *i.e.*, those formed apart from any known organismal or septic process, being deposited by a process of precipitation of certain salts from the urine, are (1) uric acid, (2) urates of soda and ammonia, (3) oxalate of lime, (4) phosphate of lime (basic phosphate of lime or bone earth), (5) carbonate of lime, (6) cystine or cystic oxide, (7) xanthine or uric oxide. The various theories suggested to account for the deposition of these salts and their aggregation to form calculi. must not detain us now. When we are asked why calculi should form, or what causes calculous formation, we have to admit we do not precisely know, though we may be able to explain more or less how calculi are formed. It is comparatively rare to find a pure primary calculus, since by the time a primary calculus becomes symptomatic it has usually set up or predisposed to such changes in the kidney that secondary calculus has been to a greater or less extent superimposed.

The two most common primary calculi in the kidney are uric acid and oxalate of lime. For a long time it was maintained that uric acid was the most frequent form, but it is now clear that oxalate of lime is the most common. My own experience and observations certainly concur with this view. Sir Henry Morris ("Surgery of the Kidney and Ureter") states: "It would seem from the cases of urinary calculi removed during life which I collected and tabulated from all available British and American sources between 1883 and 1893 inclusive that other surgeons, like myself, have been unwilling to have the trophies of their surgical operations broken up for chemical analysis. Hence it happens that out of 203 published cases the chemical nature of the calculi is only mentioned in seventy-seven. Of these seventy-seven calculi, thirtyfour were oxalate of lime and only seventeen uric acid; of the rest, thirteen were phosphatic, two cystic oxide, one carbonate of lime and ten mixed calculi. The calculi thus classed must be understood as consisting chiefly or entirely of the substances named, minute portions of other substances not being taken into account. Mixed calculi are those which are composed of two or more salts in appreciable though varying quantities. By these figures it appears that oxalate of lime calculi were by far the commonest renal concretions, occurring in a fraction over forty-four per cent.; that uric acid stood a low second on the list, occurring in only twenty-two per cent.; while phosphate occurred in 16.8 per cent.; and the mixed calculi in nearly fourteen per cent. Cystine and carbonate of lime were very rare. It may be thought that in discussing thus briefly this question I am going outside the terms of reference for this discussion. But the chemical variety of a renal calculus has much to do with the diagnosis of calculus especially from the point of view of the evidence afforded by X-ray examination, for a pure uric acid calculus offers nearly the same degree of resistance to the passage of the X-rays as the surrounding tissues; hence it may throw no demonstrable shadow, while an oxalate of lime throws the densest shadow of all. Moreover, pure uric acid calculus with no other admixture at all, is rare; hence we cannot frequently assume that in the absence of any positive X-ray evidence a stone yet exists—always assuming that the X-ray examination is conducted by an expert. But we must revert to this again. Pain and hæmorrhage are the cardinal symptoms of renal calculus; but, as we shall see later, renal calculus may exist without causing either pain or hæmorrhage, and these relatively latent or quiescent cases are certainly more frequent than is generally supposed.

The pain of renal calculus may be either fixed or referred. Aching pain in the loin, aggravated by exercise, and relieved by rest, and often by weak pressure in the loin, is the commonest symptom. A patient of my own, who had pelvic calculus for twenty years, invariably stood with his right hand grasping the right lumbar region; the kidney was thereby fixed to a certain extent, and the pain relieved. Hurry Fenwick has pointed out that especially in the fixed or embedded calculi, the patient is forced to sleep on the side of the stone, and cannot turn over to the opposite side without increasing the pain. This symptom is a frequent, though not a constant one, and when present is of great diagnostic value. But, in addition to the more or less constant aching in the loin, with occasional exacerbations, there are the cases of typical renal colic, where intense pain radiates from the kidney into the groin, testicle, or the labium majus. The distribution of the pain in the renal calculus may be very widespread, extending not only to the regions mentioned, but also to the inner side of the thigh, the leg, and the sole of the foot. The pain is almost always aggravated by exercise, and relieved by rest.

The vexed question of reno-renal reflex pains must be considered, *i.e.*, pain in one kidney being caused by the presence of a stone in the other. This may unquestionably occur, though perhaps not very frequently. Sir Henry Morris states: "I consider the doctrine of the reno-reflex with an absence of pain in the affected kidney as unproved and unsound, and if acted upon in practice, likely to lead to very serious and dangerous results."

The following two cases of my own illustrate the difficulties presented by this supposed reno-renal reflex, and are worthy of consideration, as at any time one may be confronted with the same problems.

About five years ago I was asked by Dr. Pershouse, of Johannesburg, to see a gentleman, aged forty-four, who had intense left renal colic of two days' duration. When I saw him, this attack was passing off, but he still had pain and tenderness in the left renal region. There was a history of renal pain on the right side extending over twenty years, and the passage of fragments of calculus, pus in small quantity, and blood. Whenever he walked fast or ran he had hæmaturia, though, strangely enough, he could cycle with impunity. Lumbar percussion on the right side elicited a dull aching pain. The diagnosis was right renal calculus of large size, and probably pelvic. X-ray confirmed this, and the skiagram is here for your inspection. The patient was operated upon, and I removed the large three-ounce calculus from the right renal pelvis, which I pass round. No calculus was passed after the attack of renal colic for which I was asked to see him, and he has remained perfectly free from all urinary symptoms during the five years that have elapsed, and he is an exceptionally active man to-day. This case can, I think, be taken reasonably to be one of true reno-renal reflex pain.

Another case, seen a few months ago in consultation with Dr. Cockerton, of Middelburg, Transvaal, illustrates a similar condition, but, I regret to say, without the same happy issue. A man, aged forty, had intense right renal colic for a week. He was too ill for removal to Johannesburg for X-ray examination, and also he had a recent fracture of the leg which further precluded any removal of the patient for X-ray investigation. When I saw him he was suffering intense pain referred to the right side, and there was marked tenderness and rigidity of the right renal region. The left kidney region was also tender but there was no pain on that side. There was a history of renal pain and occasional colic on the left side of two years' duration, with the passage of blood, pus and fragments of calculus. The patient was passing a fair quantity of urine, but his breath had a urinous odour, and he had headache, thirst, and furred tongue. His condition was one of impending uræmia. Under these circumstances, what was best to be done? His general condition, in the opinion of Dr. Cockerton and myself, forbade any exploration of both loins.

Sir Henry Morris states: "If pain is a symptom the question of exploration has to be considered. It should be an invariable rule to explore the kidney on the painful side. If pain occasionally occurs on both sides, but is more frequent, severe, and persistent on one side, and never occurs on the opposite side without being present on both, the kidney of the side where there is most frequent and most severe pain is the kidney which should be explored." In the case under consideration, there was, when I saw the patient, no pain on the left side, but intense pain and muscular rigidity on the right side. I decided to explore the right loin. A large, perfectly healthy and apparently hypertrophied kidney was found, and neither by palpation nor by needling could any calculus be felt. I hesitated to open the kidney for digital exploration, on account of the threatened uræmia, and his general condition made exploration of the left loin in addition at that time a very serious risk. Nothing further was therefore done, and the patient died uræmic five days later.

At *post mortem* the right kidney was proved to be a healthy, non-calculous, hypertrophied kidney. The left contained large phosphatic stones, and was practically destroyed by pyonephrosis.

This, again, can reasonably be taken to be a case of reno-renal reflex. Had an X-ray been possible, and left calculus demonstrated, as must have been the case, I should unhesitatingly have operated on the left kidney. Whether, considering his already uræmic condition, the result would have been favourable, it is, of course, impossible to say; but the case illustrates only too well the difficulties which this type of case presents, also the paramount importance and value of X-ray examination. It is possible that cystoscopic examination would have revealed the true condition by showing a normal right and a dilated left ureteral orifice; but, again, his impending uræmia made any instrumental passage of the urethra a grave and, possibly, unjustifiable risk.

The hæmaturia of renal calculus is not often profuse, nor, if we rely upon naked-eye examination only, is it by any means a constant symptom. On the other hand, careful microscopical examination will usually reveal the presence of red blood cells.

Renal calculus hæmaturia may have no relation to pain caused by the calculus, though, usually, if there is pain there is also blood. There is a much more constant association between exercise and the presence of blood. It must be remembered that hæmaturia is a symptom of many renal conditions besides calculus, and that hæmaturia associated with lumbar pain may not necessarily be renal in origin at all; I have on several occasions seen hæmaturia and lumbar pain caused by vesical growths situated near the ureteral orifice which have caused more or less ureteral obstruction and consequent lumbar pain.

In this country hæmaturia at once suggests bilharzia, and perhaps we are a little too apt to assign to this cause hæmaturias which we cannot otherwise readily explain. Renal pain, and even true renal colic, may not infrequently be caused by bilharzia. The following case will serve to show how very closely bilharzial infection of the kidney may simulate renal calculus. About five years ago I was asked to see in consultation a gentleman, aged eighteen, who had the unusual symptoms of cystitis extending over several months, and who gave a definite history of bilharzia of many years' duration. No ova or embryos could be found in the urine, even after repeated examinations, but the cystoscopic picture was typical of the effects of bilharzia. Treatment sufficed to cure the cystitis. A few months later, however, this patient had intense unilateral renal pain and typical renal colic. There was tenderness of the kidney both on palpation and percussion. The clinical picture was characteristic of renal calculus. Since drugs failed to give anything more than transient relief, the patient, though assured that in all probability—in fact, almost certainly—he had no stone, insisted upon operation The affected kidney was cut down upon, opened up and explored with the finger through a cortical incision. Complete relief, which has been permanent, followed.

In the blood which stained the urine after the operation swarms of bilharzial ova were found, though none had previously been demonstrable.

The occurrence of latent renal calculus is more frequent than is usually supposed, the systematic use of the X ray clearly demonstrating this. Sir Henry Morris states: "I have met with cases in which a calculus has grown silently to such a size as to be felt on palpating the abdomen, without causing any subjective symptoms." Bruce Clark has recorded that in thirteen out of twenty-four *post mortem* cases of renal calculus there had been no symptoms during life. Murray, of Cape Town; Noble, of Philadelphia; and others, and quite recently Doran, of London, have published cases of painless pyonephrosis and long-standing renal calculi which have caused no symptoms.

A specimen which I show here illustrates this point very clearly, also another that stress should be laid upon, *viz.*, that calculi may remain latent and symptomless until some infective disease, such as enteric or influenza, superadds a septic condition which renders signs and symptoms active. This right kidney was removed from a gentleman, a member of our profession, who came to me some four years ago suffering from profuse pyuria, one-third of the urine being pus, and right lumbar aching. At no time had he any symptoms typical of renal calculus, although it is evident that the huge uric acid calculus, still *in situ* in the kidney before you, must have been in existence many years. The most that he had ever complained of was a backache after a long day's work. The urine showed nothing until after an attack of enteric fever, when it became increasingly purulent. The case was stated by a distinguished urinary surgeon in London to be cystitis, due to colon bacillus infection. As the event proved, the cystitis, such as it was, was secondary to right pyonephrosis. Pus could be seen with the cystoscope pouring down from the right ureter. The patient has remained well ever since the nephrectomy was done.

There can be no doubt that the huge calculus in this case, in all probability, had been latent for many years, and it was only the infection caused by enteric which had stirred it into symptomatic life. The destruction of kidney tissue was, as you see, complete. One may, I think, lay it down that, granted renal calculus, the less the symptoms the greater the kidney destruction.

The specimen which I now show is from a patient who exhibited no definite signs or symptoms of renal calculus till after an attack of influenza, though there can be no question of the pre-existence of the renal condition. The calculus in this case was of a soft paint-like consistency, and caused intermittent pyonephrosis. There is, as you see, great destruction of renal tissue. The question of latent renal calculus is one of extreme importance, and I therefore quote one other case from Mr. Hurry Fenwick's book, "The Value of Radiography in the Diagnosis and Treatment of Urinary Stone": "A man, aged thirty-three, was sent to me by Dr. Cursham Corner, in September, 1902, with fixed, dull, aching pain in the left loin for fourteen days. The pain was not relieved by rest. The urine was thick with pus. There was no irritability of the bladder, no pain on micturition.

Neither kidney could be felt, but the left one was tender on deep pressure. He was X-rayed in the routine, and a huge, dense and sharp shadow was noticed occupying the left kidney area. The patient was again cross-examined, and admitted having had five attacks of left renal pain in his life, the same as he now experienced, but less severe in character. They had always occurred in the left loin. The first attack was twenty years ago, the last two years ago. On opening the peri-renal area, the kidney was found pushed high up under the ribs, and firmly embedded in hard fat and the pelvis occupied by a large stony mass. It was immovable, like a sunk rock. All along the ureter, which was also fixed, but small and thickened, were masses of densest adherent fat. Finding the pelvis much inflamed and the kidney a mere shell, I removed the kidney and pelvis, being forced to utilise the scissors freely to separate the dense adhesions. The stone weighed six and a half ounces; it was pure oxalate of lime, and its shape corresponded to the shadow seen in the X-ray photograph. Patient left hospital for a convalescent home on the east coast, but I heard subsequently he had at attack of nephritis and that albumen had appeared in the urine."

Had it been possible to X-ray the case when pain first occurred, though there was but a suspicion of stone, how much might have been saved!

It is necessary to consider the occurrence of bilateral renal calculus. Two different conditions have been spoken of as bilateral renal calculus, *viz.*, simultaneous bilateral calculus and consecutive bilateral calculus—*i.e.*, where calculus has existed on one side and been removed, and subsequently has developed on the other. A distinction should be drawn between these two.

Sir Henry Morris (" Surgical Diseases of the Kidney and Ureter "), writes: " Legueu and Albarren agree with him—state that one or several calculi are found in both kidneys in nearly fifty per cent. of cases. In seventy-six instances in which the state of the two kidneys is described, Legueu found that there were calculi in both kidneys in thirty-eight. Figures which are taken from *post mortem* records, however, are calculated to give an undue proportion of bilateral cases. Many such cases meet with their death from calculous anuria, and it is not surprising that when one kidney has long been destroyed by calculous disease, the other kidney, working alone in a person with a strong tendency to lithiasis, should subsequently produce a calculus. Figures drawn from my own operations (nephrolithotomy, nephrotomy and nephrectomy) for calculus yield a very much smaller proportion of bilateral cases, not more than ten per cent." It is not quite clear whether these cases forming the ten per cent. are cases of simultaneously occurring calculi in both kidneys, or whether, included therein, are cases in which, calculi having been removed from one kidney, stones have subsequently developed in and been removed from the other. Other sources of statistical information on the point are not at present available. One may, however, conclude that not more than ten per cent. of cases occur, presenting calculi simultaneously in both kidneys. That one must always be on the outlook for the occurrence of bilateral calculus is well illustrated by the following case, which also affords a striking example of latent calculus on one side. In May, 1909, a man, aged fifty-five, was under my care presenting the following history. As a child he suffered from bilharzia, and passed blood from time to time till he was twenty years of age. From that time till 1902 he had no urinary symptoms, and was in good health. In 1902, while micturating, he had sudden arrest of the stream with penile pain. A catheter was subsequently passed, and he states that urine and gravel were removed. Six weeks later he had much pain in the left renal region, with typical renal colic radiating into the left groin and testicle, and he passed five small stones. Till November, 1908, he remained well, and then he had another attack of the left renal colic, and again passed a small stone. In January, 1909, he had a further left renal colic, but no stone was observed to pass. When I saw him for the first time in May, 1909, the patient had persistent left lumbar aching, with an occasional radiation of pain into the groin. There was no pain on micturition, but urgency to micturate when the desire to do so came. An aching pain was elicited on left lumbar percussion. He had never had any pain on the right side, but a dull, aching pain was felt on right lumbar percussion. The urine was neutral, or faintly acid, and contained pus and blood, but not in large quantity. In amount, the urine was normal, and of fair specific gravity. An X-ray screen examination was made by Mr. Kitchen, at that time radiographer to the Johannesburgh Hospital. On the left side a large but indefinite shadow could be seen in the renal region moving with respiration, but the lack of definition did not permit of positive statement that stone was present. The shadow was more characteristic of a pus-containing cavity. On the right side three sharply-defined shadows could be seen in the renal region moving with respiration. As the left side was the one on which severe pain was complained of, it was decided to operate on that side first. On May 10th the left kidney was exposed by an oblique lumbar incision. The organ was elongated and boat-shaped, and fairly adherent to the surrounding tissues. The cortex was thinned out and pouched back from old back pressure, but a fair amount of secreting tissue remained, which, in view of the condition of the opposite kidney, it was essential to conserve to the utmost. On incision through the cortex the finger entered a considerable cavity containing pus, minute fragments of uric acid, and a phosphate-covered slough. These were removed, and the cavity irrigated out. A ureteral sound was passed, and the ureter found to be free. The kidney and peri-renal space were drained. The patient rallied well. There was free escape of urine from the drainage tube, and also free passage of urine by the urethra. On May 29th he was again examined by the X-ray screen. There was no shadow on the left side, but shadows showed as before on the right side. The sinus in the left loin was still discharging freely, and the urine passed *per urethram* contained pus in obvious amount. There was occasional aching in the right kidney. On June 1st, three weeks after the first operation, the patient's general condition being good, the right kidney was exposed by an oblique lumbar incision. The organ was much hypertrophied, and showed no evidence of back pressure. In a distended calyx one stone the size of a small walnut and several smaller stones were found. All consisted mainly of phosphates. The pelvis and ureter were proved to be free. Subsequent progress was uneventful. A sinus persisted on the left side till the end of July, and then finally closed. The persistence of this sinus is hardly to be wondered at when the pouched condition of the kidney is considered. The patient left hospital well on August 12th. He has remained perfectly well since, and has returned to his work as a carriage upholsterer.

It is quite impossible, within the time at our disposal, to do more than briefly touch upon some points in the differential diagnosis of renal calculus. In this country it must have happened to nearly every practitioner to come across cases of bilharzia which have exactly simulated renal calculus. The presence of the parasite and a negative result of X-ray examination enable a diagnosis to be made. But many of us must have, either deliberately after a diagnosis, or for exploratory purposes only, cut down upon kidneys of this character. As a result of drainage of such kidneys all symptoms as a rule entirely disappear, so far as my own experience and that of many practitioners to whom I have spoken of the matter are concerned. Such cases as a rule are not difficult to exclude.

The condition which is often extremely difficult to differentiate is tubercular disease of the kidney. Here lumbar pain, tenderness on percussion and palpation, typical renal colic, hæmaturia and pyuria, may all be present. Even the X ray may show a shadow due to caseous or more or less calcified foci. The only positive evidence is the finding of the tubercle bacillus either by microscopic methods or the inoculation of a guinea-pig. The fact that rest does not abate the symptoms, and the occurrence of frequency of micturition at night, should always rouse suspicions of renal tubercle. It must be borne in mind that many of these renal tuberculosis cases occur in people who look robust, and who present no definite history of tubercle, either family or personal. The following case shows the extreme difficulty in diagnosis which may sometimes occur, always bearing in mind that when this case first started X rays had not been discovered.

In 1894 a gentleman, then aged twenty-two, noticed his urine was thick. He had no pain. He consulted in turn four distinguished surgeons in London. Two advised kidney exploration, and a third treated him for albuminuria, cause not stated. A fourth, having had him under observation for several weeks, washed out the bladder regularly, and after cystoscopy, urged no operation, and advised the patient to reside in South Africa. Obviously the impression was that he had urinary tubercle. The patient came under my observation in September, 1907. He had passed pus all these years, and was emaciated to a degree, though he had remained fairly well till shortly before this time. Three months previously a swelling had been found in the right loin, which was aspirated and drained by a piece of catheter passed in through the canula. No tubercle bacillus had been found, but streptococci had, and antistreptoccic serum had been used with some benefit. When I saw him, one-third of the urine was pus, which was proved by cystoscopy and segregation to come from the right side. The left kidney was functionating adequately. The right kidney could be felt greatly enlarged, but was not tender. Nothing could be felt on the left side. The kidney which I now show was removed. It contained one huge calculus, which had ulcerated through into the peri-renal tissues, and several smaller calculi. The whole organ, as you see, was little more than a pus sac. I regret to say that patient died from sepsis and exhaustion seventeen days after operation. Had the case been operated on before the stone had ulcerated through into the loin space, there is little doubt the patient would have been saved. As it was, a nephrectomy under the worst possible conditions of chronic sepsis had to be undertaken. An X ray at any time must have revealed the condition, and, if done early enough, would have enabled nephrolithotomy to have been done with little or no risk.

To discuss here the value of X rays in renal calculus would occupy too much time, and would need a special discussion to itself. With its paramount importance everyone must be in agreement.

As far as my own experience goes, in no case in which the X ray has revealed a shadow which was interpreted as stone has a stone or stones failed to be discovered at operation; and, on the other hand, in those cases where no stone was revealed by X ray, and operation was subsequently undertaken, no stone was found. This does not, of course, mean necessarily that in all cases where X ray was negative and no operation was done that no stone was present. But, granted that X ray work is conducted by an expert and interpreted by an expert, the failures must be very few indeed. Failures are bound to occur in the hands of the amateur or the man of small experience. It is difficult to see how, if an X-ray shows the spinal column, the lower ribs, the iliac crest, and the outline of the psoas muscle, any stone can well escape detection. At the same time, this may be the case, as the prints from certain skiagrams of a recent case show. The bones showed up clearly and sharply in the negative, but no renal shadow was seen; nevertheless, this small uric acid stone was there, and was only manifested by the use of a soft tube, when the shadow, as you see in the third print, showed clearly. It was also visible with the screen. All the skiagrams were taken by experts in the work. Clinically the case was quite obscure, and had been diagnosed in London as some intestinal condition.

Personally, I have found both the screen and the photographic plate useful. Provided one's eyes have been accustomed to the darkroom, and the patient's loin is not too thick, the screen gives reliable information, the movement of the stone shadow with the excursion of the kidney in respiration affording confirmatory evidence, and the blurring of the shadow on a plate which movement of the kidney necessarily involves is eliminated. Mr. Hurry Fenwick, in his valuable book, "Radiography in Urinary Stone," in a chapter on failures with the X ray, draws attention to an unexplained phenomenon: "The form of failure which seems to me to need careful and searching examination is the action of certain inflamed kidneys upon the light. I do not wish to theorise, or even to attempt an explanation, but I will state that some large branched stones are apparently rather translucent to the X rays when covered by an inflamed kidney or by condensed fat, or surrounded by pus, and are thus missed even by expert radiographists. Some years ago Mr. Horace Townsend, of Cork, requested me to give an opinion upon a large, painless right renal tumour. The patient was a lady, who was passing much pus in her urine. I had the patient radiographed in London by a capable man, and saw the negative. It was denser in the region of the tumour than elsewhere, but there were no shadow details pointing to stone. An enormous branched stone was found, and removed by Mr. Townsend. He promptly took the stone, and being a keen radiographer himself he placed it under the opposite healthy loin, and X-rayed the loin, with the result that a fine shadow of a branched stone, exact in every detail, appeared on the negative." Enough has been said to show that serious difficulties may occur with X-ray examinations, and that it is of primary importance that X-ray examinations should, whenever possible, be done by experts in that line of work. The mere possession of an X-ray apparatus and a slight knowledge of technique is by no means adequate equipment for the satisfactory radiography of the renal region.

I have no intention here of dealing with operative methods, but I would suggest the following points as specially worthy of discussion in relation to renal calculus:—

1. Granted symptoms of renal stone and a negative result of X-ray examination, to what extent is purely medical treatment justified, and when should one explore the kidney?

2. Granted renal calculus on one side, and a healthy opposite kidney, what are the conditions which would justify nephrectomy rather than nephrolithotomy? I have left a kidney with a comparatively small amount of secreting tissue and much destruction, as in the bilateral case before quoted, with the best results, and, on the other hand, a much less damaged kidney has subsequently demanded nephrectomy on account of pain. Is this due to some difference in the infective process in the two types of case? Objectively and microscopically they were similar in character. Is it due to difference in the relationship and effect of the resulting cicatrix in the kidney leading in the one case to some degree of back pressure which is absent in the other?

It is interesting and gratifying to recall the fact that although Sir Henry Morris was the first surgeon to suggest deliberately opening the kidney for the exploration of calculus, our friend, Sir Kendal Franks, who is here to-day, was the first surgeon actually to perform incision of the renal parenchyma for this purpose. In a case (*British Medical Journal,* March 31st, 1888) in which there was absolutely nothing abnormal to be felt in the renal pelvis, and no hardness or inequality in the parenchyma to guide him as to the presence or position of a calculus, he made an incision of about one and a half inches long in the convex border of the kidney, and thus detected and opened a small abscess cavity in which was contained a calculus the size of a pea. The patient made a good recovery. This case formed the point of departure for nephrolithotomy to supersede nephrectomy in the majority of cases, and naturally, enormously reduced the mortality of operations for kidney stone. Calculus in the ureter is, fortunately, a relatively rare condition, for the effect of an impacted ureteral calculus upon the kidney is of a most seriously destructive character.

By ureteral calculus is meant a calculus which has formed in the kidney, but has become impacted during its attempted passage of the ureter.

If, as we have seen, the symptoms of renal calculus are frequently misleading, those of ureteral calculus are even more so. Generally speaking, the symptoms are those of renal calculus, and need similar differential diagnosis, or, when the impaction is low down in the intravesical portion of the ureter, the simulation of vesical calculus may be almost perfect. Careful X-ray examination is more than ever essential in ureteral calculus, and the systematic passage of a ureteral sound when the kidney is explored is of paramount importance, more especially when dilation of the pelvis makes ureteral stenosis at some point, in the absence of other causes of back pressure, certain. The point of impaction of the calculus may be anywhere in the course of the ureter, but from two to two and a half inches from the renal pelvis, at the entrance of the ureter into the bladder wall, and at the brim of the bony pelvis, are the most likely positions of arrest. The use of X rays will usually enable an accurate location to be made, and thus the huge exploratory incisions, till recently so common, can often be avoided, and the operation become a relatively simple one. If there are fallacies and difficulties in the radiography of renal stones, there are still greater ones with ureteral calculi. To discuss the fallacies fully would occupy more time than can be devoted just now. Mr. Hurry Fenwick has very tersely and wisely laid down the following rules:—

(1) The shadow should be in the line of the ureter; (2) it should be oval and more or less elongated in outline; (3) its long axis should be directed downwards towards the bladder; (4) the clinical history should reveal intermittent renal suffering.

Dealing with the visual fallacies in the radiography of urinary stone, Mr. Fenwick states that the conditions which are mainly responsible for the appearance of fixed false shadows (mimetic shadows) upon the radiographic negatives are, in their order of frequency:—(1) Phleboliths (calcareous infiltration of a thrombus); (2) lymphatic glands undergoing calcareous changes; (3) patches of atheroma in vessels; (4) appendical or intestinal concretions.

The only certain method of excluding these possibilities of error is by the passage of a shadowgraph bougie into the ureter by means of the ureter cystoscope, when the skiagram will show such shadows to be extra-ureteral.

The chief points that should lead to the diagnosis of a phlebolith are:—(1) A circular white spot on the plate; (2) a sharply-cut rounded border; (3) bright homogeneous surface.

The direction of the common axis of multiple shadows should often make the matter a certainty (Fenwick).

Time does not permit of any discussion of operative methods. But it should be borne in mind that, if a patient has been in the habit of passing calculi, the chances that he may pass one detected in the ureter by X rays are considerable, and some delay and medical treatment may often be desirable and permissible. A calculus detected by means of the cystoscope to be arrested at the vesical opening of the ureter will not infrequently be found to pass on into the bladder. I have seen two or three instances of this.

Vesical calculus is comparatively infrequent in South Africa, so far as my own experience and that revealed by the statistics of the Johannesburg Hospital are concerned.

The diagnosis of bladder-stone presents few of the difficulties associated with calculus in the higher portions of the urinary tract. The sound, the cystoscope, and, in cases of small stones undetected by these means, the use of the evacuator, as advocated by Mr. P. J. Freyer, will suffice always for a diagnosis to be made. Pain, frequency, hæmaturia aggravated by exercise, and sometimes the sudden arrest of the urinary stream, are the cardinal symptoms. In the cases where pain is referred to the loin, cystoscopy will clear up any doubts in diagnosis. The question of treatment appears, in this country at all events, to be one admitting of more controversy than the treatment of stone in other portions of the urinary tract. Almost invariably suprapubic lithotomy is done. But I would strongly urge the claims of litholapaxy to be considered the operation of choice in all suitable cases. The suitable cases are certainly in the majority, and if diagnosis be made early, that majority must be vastly increased. I hand round specimens of stone, cases of my own, dealt with by litholapaxy. They vary in weight from one and a half ounces to a few grains. In none of them was the patient in bed more than ten days; in the smaller stones he was up and about within three days. In none of them has there been any recurrence. This certainly compares favourably with the usual results of lithotomy, where, if the patient is about in a month, he has done well. Mr. P. J. Freyer, who has done so much to perfect the operation of litholapaxy, states: "I have now practically abandoned lithotomy in my practice in favour of litholapaxy, the force of which remark will be seen when I state that amongst the last three hundred cases of stone operated on by me in patients of all ages, from two to ninety years, the calculi weighing from two grains to six and one-eighth ounces, there were only six lithotomies (one suprapubic, four perineal, and one vaginal), the modern operation having been found feasible in all the other 294 cases."

The mortality in skilled hands is less than that of lithotomy. In 1902, fifty cases of litholapaxy were performed at St. Peter's Hospital, London, with one death.

Encysted calculi are, of course, not amenable to litholapaxy. This three-ounce encysted oxalate stone I send round could only be dealt with by lithotomy. It was caught by a lithotrite, but could not be raised from its bed. Its extreme hardness, too, would render it unsuitable for litholapaxy.

The large phosphatic calculus, three and a half ounces, shown, was also encysted. Had it not been, litholapaxy could readily have been carried out.

Excluding encysted calculi and extremely hard and very large calculi, litholapaxy is really limited only by the experience and dexterity of the surgeon.

If, however, the surgeon has had no opportunity for seeing and practising litholapaxy, there is no question he is far safer in the performance of lithotomy. But the art of litholapaxy is not a very difficult one to acquire, provided the surgeon has reasonable opportunities and some aptitude in the manœuvring of intravesical instruments.

The advantage to the patient of litholapaxy, in the saving of time and the avoidance of the miseries of a leaking bladder, are too obvious to need emphasis.

Urethral stone need not detain us. I show a small, hard oxalate, removed from behind a urethral stricture. As a curiosity, and unique in point of size, I show this phosphatic calculus, given to me by Dr. George Turner, of Johannesburg, removed from beneath the prepuce of a native in the compound there recently.

One of the terms of reference for this discussion is the incidence of urinary stone in South Africa. No extended statistics are, so far as I know, available. The following figures, kindly compiled for me by Dr. Girdwood, House Surgeon, Johannesburg Hospital, bear out the relative frequency of renal calculus and the rarity of vesical calculus.

Since May, 1905, there have been nine cases of bladder-stone, fourteen cases of kidney-stone. If other hospitals would give their statistics, some fairly sound information as to the incidence of stone in South Africa might be forthcoming.

DISCUSSION.

The PRESIDENT expressed his cordial appreciation of the paper. Stone was clearly not greatly prevalent in South Africa. Litholapaxy was the best operation, provided that the operator had had ample experience, but otherwise results were bad. Personally he had not done litholapaxy. X rays were essential in the diagnosis of renal calculi. He warned surgeons against using the word "explore" to patients; it frightened them terribly. Illustrating difficulties in diagnosis, he related a case in which a renal calculus existed but which some one else treated by removing the appendix. His experience of ureteral stones was small, but he had removed one. He related a case in which, operating for a renal calculus, he found the kidney absolutely degenerated, nothing but fat, death resulting. He briefly discussed the incision and removed operations, and concluded by saying that he had not seen many vesical calculi in South Africa.

DR. W. G. ROGERS reminded the Section that Johannesburg owed its position in renal surgery to the work of Mr. Temple Mursell. Patients were always asking as to the cause of renal calculi, and he generally told them "the passage of micro-organisms." He thought limestone formations had a good deal to do with stone, as a lot of cases came from the Free State. He quite corroborated the difficulties of early, and even of late, diagnosis. Related a case of persistent pain in heel set down to rheumatism or arthritis, in which, pain in the groin suddenly supervening, attention was directed to kidney, stone diagnosed, kidney removed, and the heel pain entirely disappeared. Another case illustrated the importance of having X-ray work done by an expert. On examination no shadow was discovered, but patient went to England, and after obtaining a multiplicity of opinions, an X-ray expert shadowed a stone, and an oxalate calculus was removed. Another case illustrated the existence of calculus without symptoms. A man who was perfectly well himself accompanied a friend who was to be X-rayed. Nothing was revealed in the patient, but the other man jocularly said "You might just as well look at me"; and a big shadow was seen and a calculus was subsequently removed. Bilharzia frequently gave symptoms suggesting renal calculus, and these symptoms were often completely removed by a renal operation. G. A. Turner reported frequent cases of Natives keeping steadily at work with calculi. Lithotomy was formerly the general operation done in Johannesburg, because they had not the necessary experience for crushing until Mr. Mursell came along. Crushing was infinitely more satisfactory but infinitely more difficult.

DR. DANEEL pointed out the errors that may be made in diagnosis by X rays. Even in expert hands, shadows have been interpreted as stone which really represented tubercular or polycystic kidney degeneration, calcified glands, ossifications in ligaments or foreign bodies in intestine. Neuhauser publishes two cases from Israel's clinic, where shadows were present but no stone found on operation, and where microscopic examination of excisions from the kidneys revealed nothing to explain the shadows. Neuhauser suggests that certain minute changes must have been present in the tissues to cast these shadows. Chromocystoscopy sometimes rendered excellent service in obscure cases, as either during the attack or even afterwards, marked changes in the functional capacity of the affected kidney could be detected. With this method very often the kidney could be proved to be the seat of trouble in colicky abdominal pains. In very rare cases it happened, as Israel has pointed out, that colicky pains originating from the kidneys might co-exist with free secretion from these organs. With stones in the ureter, just above the entrance into the bladder, there were sometimes marked changes at the ostium, as a rule a slight prolapse of the ureteral mucous membrane into the bladder. In cases of anuria an operation should be performed within the first forty-eight hours. In deciding on the operative incision, special care should be taken to individualise each case. If possible, a nephrotomy should be avoided, but where it should prove necessary one need not dread it to the same extent as formerly, as Novikow has proved that the functional capacity of the incised kidney rights itself again after some time.

Dr. Waldron was extremely grateful for Mr. Temple Mursell's paper. He remembered one case, a big man who had persistent pain in the heel, diagnosed as referred pain from kidney. Was treated with solvents without result, but, on his passing an oxalate stone *per urethram,* the pain disappeared. He agreed that unless one is careful to secure a really competent expert, X-ray diagnosis may bring one much discredit. A case of his had painless hæmaturia after rowing, but never at any other time. Possibility of stone was thought of, but nothing definite made out. Six radiographs were taken without the slightest shadow, even when a horseshoe pad was applied and exposure effected intermittently with deep inspirations. The hæmaturia persistently recurring, the patient went to a surgeon in England, and a radiograph taken there very clearly shewed a stone, which was removed.

Dr. Caiger showed photographs and gave some details of a case operated upon by Dr. Raubenheimer, remarkable for an unusually large number of comparatively large stones (reported in *S. A. M. R.*, August, 1910).

Dr. Balfe said that the Addington Hospital records only gave six vesical calculi and eight renal in fifteen years. He agreed as to the difficulties in diagnosis. Bilharzia frequently caused renal colic, but why he did not know. He related a case of a boy with bilharzia and severe lumbar pain, apparently renal. The kidney was explored with no result. The pain continuing severe, he went to England, and, after several varying opinions an Assistant Surgeon at a London hospital removed his appendix and all pain disappeared.

Dr. Simpson Wells also expressed his sense of the value of the paper. He related three cases in a series, all in young men in full vigour, all with lumbar pain, pyuria, hæmaturia and oxalic urine. All three were relieved by passing stones, the only treatment being urotropine.

Dr. Howell Davies spoke of the association of renal colic with bilharzia. In the Pretoria Hospital statistics there were only nineteen cases of calculi out of some 1,900 patients.

Dr. Casalis spoke of the occasional association of renal calculi with uterine fibroids, and gave two cases in which, after removal of fibroid, the patient complained of being very little better, and later renal calculi were removed, with recovery.

Dr. A. Mackenzie spoke of bilharzia often causing vesical irritation, sometimes with only slight hæmaturia, and with renal colic later. Very often a calculus formed round bilharzia. Microscopical examination had value in such cases. Did not often operate in these cases because the irritation was generally diffused and only at a later stage does a stone form. He related a case of renal colic always following exertion and with a trace of albumen and a few blood corpuscles during the attacks. He advised operation, but the patient preferred going to England, and saw a man who told him he had no stone. He had a long rest on both voyages and apparently got well, but he (Dr. Mackenzie) stuck to his opinion of there being a stone, and when the patient returned to his usual habits the symptoms recurred, and eventually he was operated on and a stone removed. The X rays were of great use now-a-days, but the absence of a shadow was not absolutely conclusive, unless one had a first-rate machine and a first-rate operator. He related a case in which no calculus was found on operation. Later, Davidson, in London, found a small shadow, and the patient was again operated on and a stone removed. The first operator had trusted to needling, and had not incised. He was much impressed with the necessity of finding out the micro-organisms in the kidney, and giving the appropriate vaccine, as a preliminary. Suprapubic lithotomy was much easier than crushing, but a specialist with much experience would be well advised to do the latter.

Mr. Temple Mursell, in reply, said that litholopaxy was clearly admitted to be the best operation, and in suitable cases it was not so abnormally difficult as some men seemed to think. Nephrolithotomy was sometimes disappointing. He related a case where after this operation by a German surgeon with no result an oxalate calculus was found and removed. The formation of primary calculi from organisms had never been proved. He agreed with Dr. Rogers as to the frequency of calculus in the Free State. Pain in the heel referred from kidney was not infrequent, and when it existed the kidney should always be suspected. He did not know whether stone was caused by bilharzia, although the men in Egypt thought so. He agreed with Dr. Daneel as to the value of cystoscopy. One fallacy in X-ray diagnosis was caused by the patient not having bowels cleared out. He related a case in point. Salts, however, should not be given as an aperient, as a concretion might be formed which would simulate shadow. In connection with Dr. Daneel's observations of dyes, the difficulty was that one could never tell which ureter they came from. He thought actual incision of pelvis advisable where large thick stones existed, but not with small ones. You may get a curious "ballooning" of the ureter when a stone was impacted near the entrance into the bladder. Dr. Waldron's case suggested a growth, and villous growths being rare, one would have been led to think of carcinoma. Carcinoma occurs at an earlier age than is generally supposed. He was glad that litholapaxy was not attempted in the case recorded by Dr. Caiger, and he agreed with Dr. Balfe's figures as to the comparative rarity of vesical calculi. Dr. Wells's cases were interesting, and the treatment by solvents was sound, although complete natural evacuation was rare. Had had no experience of simulation of stone by bilharzia, but fortunately the operation almost always relieves the pain. He alluded to Fenwick's suggestion that manipulation in exploration causes a blood clot to form round a stone. He agreed with Dr. Mackenzie's advocacy of vaccine treatment as an adjuvant.

II.—THE SURGERY OF INTESTINAL OBSTRUCTION.

By Sir Edmond Sinclair Stevenson, M.D. (Brux.),
F.R.C.S. (Edin.), Rondebosch.

It is well to dismiss from our mind impressions which have been made in the early period of our medical education, and all the more lasting on that account, which may lead us to believe that the organs situated in the abdominal cavity have fixed positions.

We read in books of anatomy, and see in their illustrations, and in observations made on the subject in the dissecting-room, that the stomach lies transversely in the epigastrium, that the transverse colon also lies transversely in the abdomen below the stomach, and that the ileum joins the ascending colon almost at a right angle. Radiography has altered all this. The healthy stomach, when loaded with heavy food or with Bismuth porridge, may be seen to hang down almost perpendicularly towards the pelvis, dragging down the liver by means of the small omentum, kinking its pyloric end, and displacing its relations with the duodenum. The transverse colon may also be seen lying in the pelvis, there forming an acute angle, when weighed down by a growth; and the cæcum, when stretched by retained fæces or by a growth, may sink down in the pelvis and simulate diseases of the right appendage. All these alterations of position will tend to render a precise diagnosis difficult. The fact is that there are no organs in the human body which possess such a latitude of movement; and it must be so, for the alimentary canal is a hollow organ, contracting and dilating according to its fulness or emptiness, adapting itself to the positions of the body, the different positions following the movements of respiration in accordance with the length of their mesenteric attachments. In health both the small and large intestines, in accordance with the length of their mesenteries, are constantly moving from one part of the abdomen to the other; so that two loops of bowel which are in contact may at any time become widely separated.

In common with the description of all operations, a clear understanding of the anatomy of the part must be mastered before any surgical procedure can be properly carried out; I shall, therefore, direct your attention, in the first instance, to some fundamental facts in the anatomy of the bowel as surgically applied.

ANATOMY.—*The Coats of the Intestines.*—The bowel is covered with five coats, two of which are of importance to the surgeon—the submucous and the serous. It has been proved by experiment that the former is the only layer capable of withstanding the peristaltic strain on a stitch, hence the through and through suture, which for convenience is passed through all the coats, and will last for days if cleanly done, and is sufficient to prevent separation. Enough lymph is thrown out in eight hours to make a fairly sound union. It is often the case that patients suffering from obstruction are in a very exhausted condition, not having had any nourishment for some time, or if they have had some, it has been constantly vomited. The need of early feeding *per via naturalis* after operations is an important factor in favour of the patient's recovery.

The other coat is the serous or peritoneal layer; this is both important and interesting. It covers almost entirely all the organs contained in the two peritoneal sacs, and it has the special property of throwing out a lymph secretion for the repair of lesions; it is also capable of shutting off by adhesions an infected area and closing up the general cavity. But this is not all: lately we have learned that under traumatic and bacterial irritation it secrets an abundant quantity of fluid diluting any toxine present, and, further, it has the property of destroying bacterial growth. On the other hand, it may cause adhesions, which at times will be the source of much trouble. This serous membrane has had an evil reputation in the past. Every bad result was attributed to the peritoneum, but in the last few years a revulsion of feeling has taken place in its favour, and those of you who have read Mr. Barker's interesting address to the British Medical Association at Belfast last year must have wondered at the description of remarkable results after operations performed under the most adverse circumstances. Several resections of intestine for obstruction due to tubercular and cancerous disease, the bowel bathed in pus and excrement, the suture made, the parts joined together, and the abdominal wound closed without any kind of drainage—I can well understand Mr. Barker, after such results, calling the peritoneum the "surgeon's friend."

I am also entitled to have an opinion on that friend, for I have dealt with that membrane for over twenty-five years, and it will take time for me to forget the hours of anxiety and the nights without sleep which followed an abdominal section—watching with anxiety for the danger ahead. I give it credit for fighting a good fight when the enemy is in the place, but I accuse it of being especially attractive, being the best forcing ground for the culture of micro-organisms on account of its warmth, moisture, and close vicinity to the intestinal contents. If a friend, it appears to me to be a capricious one.

The Mesenteries.—The peritoneum forms the attachments which anchor the bowel to the post-peritoneal layer, and which, in accordance with their length, give the bowel the necessary freedom of movement. On reaching the bowel it divides to closely cover the intestine, in a manner similar to that of a sheet over a clothes-line. This, anatomically, is important to us—the V-shaped space at the mesenteric attachment causes a longitudinal strip of muscle to be uncovered by peritoneum. In resection of the intestine or in wounds affecting that part this must be borne in mind, and the mesenteric attachments to the bowel must be carefully stitched, great care being taken in securing a perfect serous approximation. If not, the part will be deprived of its circulation as, at the junction of the layer of peritoneum, the arteries supplying the gut terminate in the single *vasa recta* which do not anastomose, and the want of union will likely lead to local gangrene. Mrs. X., suffering from a growth of the womb, the growth being intimately adherent to the sigmoid: In the attempt to detach the tumour the serous attachment to the bowel was torn. The serous membrane at the triangular space of which I have spoken was carefully stitched, but, as you know, it is a difficult position for skilful manipulations. Some days afterwards a fæcal fistula appeared, becoming rapidly larger, so much so that the patient passed large formed motions. With proper diet and cleanliness the fistula healed.

The Ascending Colon has a short mesentery—that is, the bowel is fixed by its outer leaf, whilst the inner part is long. Its attachment is an insecure one to the posterior aspect, and upon this the weight of the ileo-cæcal coil is suspended. In cases of large cæcum, when for some reason food and gas are retained long, the weight drags down the bowel, and the result is to loosen the ascending colon. On account of the ascending colon being rendered immovable by the outer sheath of its mesenteric attachment, it would be extremely difficult to resect and drain this part of the bowel were it not that, by cutting through its outside layer, a part which contains no important structure, one is able to mobilise and turn the bowel inwards. This method of dealing with the difficulty—" the mobilisation of the bowel "—facilitates and renders the operation a comparatively safe one. I do not know who was the surgeon who first drew our attention to it; whoever he is, we owe him a grateful debt, for besides this, it enables one to deal with the ureter of that side as well as the duodenum.

The transverse colon from the hepatic to the splenic flexure has a very loose attachment, which, as I have already said, enables it to fall down in the pelvic cavity. The descending colon and sigmoid have also a fixed attachment.

The Circulation.—We know how abundantly the bowel is supplied with blood from its large trunk arteries, the branches anastomosing freely with each other by means of the loop system of circulation. But there is one part of the intestine which has quite a different blood supply. I refer to the lowest part, the rectum, and unless this is clearly borne in mind disaster may result. Professor Hartman of Paris published in the *Annals* of 1909 an interesting paper dealing with this, and his observations have been corroborated by Dietrich and others. He shows by diagram and clear anatomical statement that the inferior mesenteric artery, together with the middle colic, supplies the bowel as far as the upper rectum, and as far as that the sigmoid branches of these arteries anastomose freely, but just above the ampulla the inferior mesenteric artery abruptly terminates, like the tap root of a tree, into a single artery, the " superior hæmorrhoidal," sometimes called the superior rectal. This vessel begins in the anterior layer of the rectal fascia, where it is bound close in the bony covering, and then reaches the rectum at its upper part. If the removal of the rectum is made from below, as by the transacral operation devised by Kraske, it is extremely difficult to draw the part of the bowel down in order to sew its lower end to the anal edge, or to anastomose the proximal part of the bowel to the distal end below. The superior hæmorrhoidal artery remains fixed when the peritoneal *cul-de-sac* is opened, and it is easy to drag down the recto-sigmoid part to the anus, but the rectum proper, especially at the ampulla, remains fixed, held there by the artery. To avoid gangrene of the lower section it becomes necessary to resect the bowel above the origin of the superior hæmorrhoid, so as to bring down a part which is fully supplied with blood and sew it to the anal edge. Kraske, to whom surgery owes so much for introducing the transacral operation, has acknowledged this weak point in the operation, and has made surgery a greater debtor by devising the combined method.

Whilst operating on a case of partial obstruction in the upper rectum—obstruction due to a ring constriction, probably malignant—I had an opportunity of seeing this. Having to mobilise the descending colon and the sigmoid in order to bring down the upper segment through the healthy part of the lower rectum and sew it to the anal edge by cutting the sigmoid below the origin of the inferior mesenteric, the upper section with all its circulation could be easily brought down. Complete obstruction of the bowel may be caused by hæmorrhage. I saw a coloured man suffering from acute intestinal obstruction; he died soon after reaching the hospital. At the post-mortem, the obstruction was found to be owing to the rupture of a mesenteric artery, which caused the hæmorrhagic sac to bulge on to the small intestine, completely closing its lumen.

THE SURGERY OF OBSTRUCTION.—Having dealt with these preliminaries, I wish to say that the *surgery of intestinal obstruction is the surgery of intestinal resection plus the drainage of the bowel.*

When called upon to operate in a case of acute obstruction, the surgeon has to deal with one of the gravest and most anxious of all surgical interferences. From the outset he is met with conditions bristling with difficulties. The diagnosis in the early stage is not certain, for it may resemble symptoms due to many acute attacks of the abdominal organs, especially of those situated in the upper quadrant; and to localise the seat of the obstruction is far from being an easy matter. If he can locate the disease, what does he find? The constriction and its results, *viz.*, the strangulation of its circulation, gangrene and thrombosis, leading to rupture and septic peritonitis, and on the proximal side highly engorged and dilated coils of bowel in which retained putrid material is rapidly being absorbed, causing that most terrible complication, stercorræmia. What is best to be done—relieve the obstruction, drain, or do both? Of the two dangers the toxæmia is the more deadly; the majority of deaths are due to septic intoxication and not to peritonitis. It is surprising how draining and saline injections will help to tide over a most critical time, and enable the surgeon to proceed with the next step under far better conditions. It will add little time to the length of the operation, and is comparatively a minor additional danger. Anyone who has been witness to the disasters which follow an operation of this kind where drainage has not been done will agree with this. There can be no rule so binding to the surgeon. On the other hand, the operation cannot be said to be complete if you leave the strangulated part of the gut behind. Only careful consideration and judgment must be our guides; but to all rules there are exceptions.

If the surgeon is early in the field, which unfortunately so seldom happens, then the relief of the obstruction may be the only step required.

Mrs. A. had been operated on for pelvic trouble, and had been doing well up to the tenth day, when she was seized with severe pain and other symptoms of acute obstruction. Everything was ready for surgical interference—nurses, surgeons, instruments and locality—and no relations to interfere. The abdominal cavity was re-opened, when a curious and interesting condition of things was seen. The large omentum was twisted like a rope, and adhered by its end to the post-peritoneal wall; the small bowel was twisted twice around it; to disentangle the coil would have been a difficult task. The omentum was detached from its adhesion and unfolded, the relief of the bowel was comparatively easy, and recovery was rapid.

Let me illustrate the reverse of this.

Mr. S., a comparatively young and healthy man, went to work in the early part of the day feeling well. On leaving his office in the afternoon he was seized with severe colic, followed by other symptoms of acute obstruction. When the abdominal cavity was opened, the small bowel was seen to be dilated and engorged. The seat of obstruction was found to be in the small bowel (I don't know which portion of it), which was adherent for the length of one inch to the post-peritoneal covering below the splenic flexure; this adhesion had caused the bowel to twist on itself, causing acute obstruction. The adhesion was not a fresh one, and required some force to loosen it; the kink then rectified itself, and, on the bowel being freed, gas immediately passed through, and I imagine fluid as well. Tempted by the condition of the bowel, the short space of time occupied by the operation, and the facility of releasing the obstruction, the patient being exhausted, I closed the wound. He did well for two days, and then collapsed suddenly. Several sudden deaths have occurred after operations for the relief of intestinal obstructions, and have been attributed to thrombosis of the pulmonary artery; in some cases, however, no proofs of this could be seen at the post-mortem examination. It is more likely that those fatal results are due to stercorræmia and resulting heart failure.

Drainage.—There are many ways of draining the intestine, depending on the condition of the patient and the part of the bowel which is to be opened. If the patient is exhausted and no time can be wasted, the simplest method is to seize the most dilated coil which presents itself at the opening, and which fortunately is generally nearest to the obstruction, owing to the forcing peristaltic action against the obstruction, which results in stretching its mesenteric attachment. If you have a good assistant, he will with two fingers of each hand clamp above and below; the parts are well packed to avoid soiling the cavity, the gut is opened opposite its mesenteric attachment, the edges of the wound kept open by fine forceps and drained of its contents; the opening is then closed and dropped back in the abdomen. Later, when the patient has recovered from the shock, the removal of the obstruction can be proceeded with. If the condition of the patient allows it, a more thorough drainage is advisable. The bowel is opened between clamps, the edges of the wound are kept apart by fine peritoneal forceps, and a glass tube inserted—a Bantock or Paul, if you possess one; if not, any kind of glass tubing will do. Milk the bowel towards the opening, and draw gently the gut on to the tube as a glove is drawn over the finger. In this way, on a tube six inches long, about twelve feet of the bowel can be drawn. In cases where the double operation is safe, the resection of the bowel is performed in the usual manner: The clamps should be applied on healthy tissue above and below the obstruction, for if fixed on intestinal walls which are unhealthy in appearance, thickened, ulcerated or sodden, there is no security that the suture will hold; besides, it has been demonstrated that such a condition of the bowel leads to the migration of toxic germs into the cavity. It must be borne in mind that enterostomy is not a safe way of draining the small bowel; the drainage of that part of the bowel should not be kept open too long, for it has been observed that in such cases patients starve rapidly, and that the bowel above becomes attenuated and forms adhesions, which may leave the patient in a worse condition than before. Whenever possible, and that means when the obstruction is in the larger bowel, drain through the cæcum or the appendix rather than through the small intestine. In the larger intestine it is different; in that locality drainage can be kept indefinitely by means of a Paul's tube or by colostomy and making an artificial anus. In cases where the drainage is to last for some time these methods of draining the bowel are, I think, best.

Resection.—Resection of the bowel is generally done between clamps, but it may be done by using strong ligatures instead above and below the obstruction. It is a matter of careful consideration to decide how much of the parts should be resected. The careful examination of the parts affected should guide us, *viz.*, the condition of the walls, the colour and the condition of the blood-vessels. (The first step is to free the coil or part which we intend to remove so as to enable it to be drawn well out of the abdominal cavity to prevent soiling the peritoneum. In the large bowel it is done by mobilising, and in the small, if there is any difficulty, its mesenteric attachment is secured by tying it by means of a threaded aneurism needle and cutting it.)

The clamps are applied wide of the affected parts; any unhealthy portion of the gut left is a danger retained, for healthy intestinal walls will prevent the migration to any serious extent of the bacteria through them; but if their vitality is lowered, whether by accident or disease, even without any breach of surface, this migration will take place, and the general cavity of the peritoneum will thereby be infected. This condition is well illustrated in some cases of strangulated inguinal hernias, in which we find that, although there is no signs of rupture of the bowel, the serum which has exuded in the sac is teeming with toxic organisms. It is better to err on the side of free removal; after all, it is not the length of the bowel removed which adds to the danger, for several feet of the bowel can be as easily removed as 2 inches; the point of importance is not so much the extent of the bowel removed as the length of the bowel which remains. For the length may vary in man from 15 to 33 feet, and a patient whose length of intestine is 30 feet will be able to submit to a more extensive resection than the one whose length of bowel is only 20 feet. The whole of the large intestine has been resected in some cases with some of the small intestines without there being any obvious bad results. Physiologists are now making researches by means of chemical analysis of fæces, and what effect the removal of various lengths of the bowel has on different individuals, both as regards the length and the part resected. No doubt we shall in time get some light thrown on this question.

Anastomosis.—There are various ways of joining the resected ends of the bowel—end-to-end, end-to-side, lateral anastomosis, and by means of invagination. In some cases one may laterally anastomose parts without resecting the obstruction. This may be done in cases where the obstruction is chronic; cases where the parts are not injured, and can be left in comparative safety; and in those unfortunate malignant cases where the patient is suffering from inoperable conditions which have become acute, threatening complete obstruction.

The end-to-end junction is the ideal one, but it is necessary to ensure plenty of slack, for if there is any tension or drag, it should not be attempted. Connecting the ends is difficult—the sero-serous stitch at the mesentery is not easy, and impossible if that part of the bowel has no mesentery. The ends which are to be joined may be of a different calibre; this difficulty can be soon surmounted, but it adds time. Personally, I prefer the side-to-side anastomosis; it has none of the drawbacks which I have mentioned. The small bowel can be easily fixed to the larger; it is quick, and I think a safer method. To invaginate the proximal into the distal part, or the reverse as the case may be, seems to me to be an easy way of anastomosing the gut, especially when the small intestine is invaginated into the large. Maunsel's operation is an example of this, but it seems to be a complicated process. I have never done it nor seen it done, and am no judge, but will describe further a way to do it which appears to be preferable. The anastomosis of end to side I never have done, but it is possible that in some instances it may have to be considered. After all, the best way depends much on what you are accustomed to, in so far as it will shorten the length of the operation; for after all any details in the performance of an operation which will shorten the period of its execution may justly be regarded as a distinct advantage. Doyen, in the preface of his book on surgery, brings forth this golden maxim: "*Operez vite mais operez bien,*" and of all surgical interferences, the one performed for the relief of acute intestinal obstruction is the one where rapidity of execution is essential. On opening the abdominal cavity coils of bowel bulge at the opening and obscure the view; are there any steps which will help in finding the seat of obstruction? I am now alluding to the small bowel; the difficulty does not exist as far as the large bowel is concerned. To draw, or allow the escape of, coils out of the abdominal cavity will simplify the diagnosis; it may at times become necessary even to complete eventration, but it is not a safe step, for, besides the shock which is caused by it, the bowel is much dilated and congested. In acute obstruction the peritoneum covering the bowel is so stretched that, on looking at it closely with a lens, you will find that the serous layer is fissured and the muscular covering underneath stretched to its limit; in fact, the bowel is in process of perforation, and the emigration of toxic germs has probably commenced. Too much handling will not improve that condition. Is there any way by which the surgeon may be able to recognise what part is presenting, besides the knowledge which only a large experience can give? The recognition of the large and small bowel will be easy. A knowledge of whether a mesentery may be found or not, and the localisation of a particular loop, or the differentiation of its proximal from its distal end may be more than convenient. In its normal condition, when the small intestine is not much inflated, there are some parts in its length which will help, *e.g.*, the duodenum at its origin and at the part where it joins the ileum, that portion of it where it appears from behind the stomach, where it is anchored to the post-peritoneal part by the ligament of Treitz, from there to its junction to the ascending colon there are some distinct features which may help one in diagnosing. But if one fails in shortening the time of an operation from difficulties in locating the seat of obstruction, there remain means of operating which will materially shorten the length of it. One of them is by the process of invagination as described by A. E. Mayland.

Intestinal Anastomosis by Invagination.—The desire to execute anastomosis in as short time as possible led him to adopt that method. After removal of diseased parts, say, for excision of the cæcum, the proximal patent end of the colon is stitched and withdrawn enough to allow a longitudinal incision of sufficient length to be made through the parietes slightly above the occlusion—the length of the incision will be determined by the size of the ileum, which, in some cases, may be dilated and thickened. A large-eyed needle threaded with silk about eighteen inches in length is carried some three or four times across the patent orifice of the ileum as if to close it. The free end of the suture which has been left long is then passed through the eye of the needle, so that the ileum is now secured as by a sling. The needle is next passed with the colon through the previously made incision, and directed up the canal for about two inches, when it is made to penetrate the bowel wall from within outwards. By pulling out the needle and dragging upon the sling suture, the *ileum*, with a little guiding, is drawn through the incision and made to pass for a short distance along the canal of the colon. Whilst an assistant keeps tension upon the sling, the operator commences to fix the ileum to the colon by a series of interrupted Lembert sutures passed round the circumference of the opening of the colon. The needle is unthreaded, and by pulling upon one of the ends of the sling, the other is drawn out of the ileum. The orifice of the latter being thus left free to open out, a ligature is then tied around the puncture in the colon. In this case the mesentery of the ileum is thinner than the meso-colon, so that it can be sufficiently drawn in when traction is made upon the small bowel. But the comparative bulkiness of the meso-colon necessitates that about one inch or so of ileum structure be tied off before a sufficient length of bowel can be properly invaginated. I think that the method which I have described is a sound one, for, besides being a quick way of anastomosing, the after advantage of invagination makes it probable that nothing of the nature of contraction, much less of occlusion, will take place at the junction.

Another simple and rapid way of resecting the intestine is described by H. Lienthal, of Mount Sinai Hospital. It was performed in a case of a growth affecting the pyloric end of the stomach, but it will apply to any part of the intestine.

In a case where the cæcum was principally involved as well as the appendix, the ascending colon was tied with a double ligature of stout twine, and the ileum treated in the same way. As the ascending colon was quite firmly fixed to the posterior wall he anastomosed the ileum with the transverse colon by a simple side-to-side union. The string attached to the gut was left hanging out of the wound, and came away on the tenth day, and the man was able to leave the hospital twenty days after the operation.

When the surgeon is unable, or judges it inefficient, to resect the bowel in obstruction in the large bowel, especially when the disease is situated low down, and when it may be necessary to interfere with the anal sphincters, which would lead to incontinence of fæces, the only plan is to make an artificial anus. When the anal sphincters have been destroyed, as in the Quenu operation, it is better to have an inguinal artificial anus than one over which the patient has no control. Incontinence in that position would be much worse than in the inguinal region. Many surgeons have devised operations in order to get over this difficulty, making use of the left rectus for the purpose, such as the Mayal rectus operation of inguinal colostomy, where the muscles are split as in the House operation.

Charles Ryal, Surgeon to the Cancer Hospital, Brompton, has lately described the following method. The rectus is split vertically, and the sigmoid is drawn out and divided at a convenient part. The lower segment is closed and replaced in the abdomen. The upper segment is made less bulky by removing the appendices epiploicæ, and freeing it of its mesenteric fat without interfering with its circulation. A loop of muscle fibres is separated from the posterior aspect of the rectus on either side of the wound so that one loop overlaps the other—this forms a ring, and through this the bowel segment is drawn. Sutures are inserted to keep the muscle fibres together above and below when the bowel comes through; anchoring stitches are inserted through the skin and muscles inside to keep the bowel in position. The wound is closed above and below and the cut edges of the latter are sutured to the skin; a double sphincter is then formed consisting of long and circular fibres.

There are many cases of obstruction due to compression from outside, such as are produced by hard growth occluding the lumen of the bowel, to arterial hæmorrhage, and to certain mesenteric cysts; lastly, intestinal obstruction may be caused by paralysis of the gut, post-operative or otherwise, as the following case illustrates.

Mrs. O. had been suffering for many years from obstinate constipation, relieved only by strong purgatives and high enemata. When I saw her she was very emaciated, and had a miserable pulse, with symptoms pointing to complete obstruction. On opening the abdomen it was found that at the juncture of the transverse with the ascending colon at the splenic flexure the gut was adherent to the parietal wall. It was found impossible to liberate this without cutting through the costo-colic ligament. The occlusion was not complete, there was room for gases to pass, and the proximal bowel was not dilated above, but below the adhesion the descending colon and sigmoid were no larger than three-quarters of an inch in breadth, pale in colour, and with very thin walls. Evidently the distal part had entirely lost its power and had become atrophied. The patient recovered from the operation, but paralysis remained. She had several small motions afterwards, but died three weeks after. The proper operation would have been to anastomose the ileum to the sigmoid; it would have given the patient a better chance.

I have not mentioned in this paper cases of intussusception or of strangulated hernia. In both of these if the strangulation is complete the treatment is the same as in acute obstruction. In many others the parts are not in such a critical condition as to require drainage and resection.

III.—CASES ILLUSTRATING THE FEATURES OF HÆMORRHAGE IN DUODENAL ULCERATION.

By W. G. Rogers, M.D., M.S. (Lond.), F.R.C.S. (Eng.),
Johannesburg.

Case I.—J. A., a young man, aged 26, an accountant, of somewhat poor physique, with the exception of dyspepsia had had no previous illness. He first consulted me in January, 1908, and then gave the following history:—For several years he had suffered from what he described as chronic indigestion, the salient features of which were discomfort and pain about the epigastric region, beginning as a rule about two hours after meals and usually persisting until he again partook of food, when all his symptoms were relieved. His appetite was good, and he was quite equal to his professional work but was easily tired by undue physical exertion.

He stated that on the day prior to that on which he consulted me he was returning home from an hour's walk when, without any other warning, he felt first faint, and then sick. He vomited twice, on both occasions bringing up a quantity of blood of which some was bright red, and within a few moments of this occurrence there followed a free action of the bowels, also accompanied by dark but not black blood. For some time he felt too weak to move, but eventually managed, without assistance, to walk to his home about a quarter of a mile away.

When I saw him next day he still afforded sufficient evidence, in the character of his pulse, in the general and marked pallor of his skin and in shortness of breath on the slightest exertion, of the real severity of the attack.

He was kept in bed for three days on nothing but water, then on barley water and sour milk for a fortnight, and so slowly back to an easily digestible but restricted diet, on which he continued under observation for altogether three months. During this time he never lost entirely either the discomfort or the pain occurring at an interval after food, and on most occasions the symptom of pain could be elicited on pressure over the pyloric area.

He was therefore sent into a nursing home, and the day after admission the passage of a tarry stool indicated a recurrence of hæmorrhage. This continued for about ten days, when it ceased spontaneously and a few days later, there being no indications of further bleeding, the operation of posterior gastro-jejunostomy was performed. He made the usual rapid recovery we expect to see after this proceeding. There has been no recurrence of hæmorrhage, his digestion to-day, nearly three years after operation, is excellent, and for the first time for over six years he is in normal health. At the operation there was seen to be definite scar formation, evidenced by thickening and puckering of the wall of the duodenum just beyond the pylorus.

Case II.—Some months after this experience I was asked by Dr. Dodds, of Germiston, to see a patient, a single man, who was found in his quarters in a condition of profound collapse. He was removed to the Primrose Hospital, where he rallied sufficiently to give a history of a sudden attack, about eighteen hours previously, of acute abdominal pain; this was rapidly followed by vomiting and diarrhœa, during which he brought up and passed a large quantity of blood. He died a few hours later, and at a *post-mortem* examination made on the following day a perforated duodenal ulcer was found, early general peritonitis was present, and the stomach and intestines were loaded with blood throughout.

Case III.—Ten months ago I was called to see L. S., aged 36, who for the previous four years had suffered from persistent indigestion, in which discomfort, but not actually pain, occurred from an hour to two hours after food. Though there was an insistent demand for food of some kind between meals and relief followed the ingestion of food, the condition was not definitely one of hunger pain.

He stated that he had slept perfectly soundly until 6.30 a.m., when he became conscious of some slight abdominal discomfort and distension, and then shortly after an inclination to stool. He walked downtairs to the lavatory, his bowels acted freely, with the passage of a large quantity of dark and also bright blood. He felt faint and sick but did not actually vomit. After returning to his bedroom the feeling of faintness continued, and within half an hour of the first hæmorrhage there occurred a second one, which completely prostrated him.

When I saw him shortly after, he had somewhat recovered, though pulse, respiration and skin still gave sufficient evidence of the great loss of blood he had sustained.

For a few days he continued to pass tarry stools, which slowly first changed from black to grey and eventually became normal about ten days after the occurrence of the initial hæmorrhage.

There has been no further bleeding; the symptoms suggesting hunger pain, as well as the sense of discomfort and distension, have gradually disappeared, and to-day (November, 1910) for the first time for nearly five years his general health is excellent and his digestive system apparently normal.

February 27th, 1911.—Recurrence of gastric symptoms with marked hunger pain in December, 1910, induced him to submit to gastro-jejunostomy on January 27th, 1911, when a puckering scar was found on the duodenum. Recovery uneventful. Digestion excellent.

Case IV.—The fourth of this series of cases occurred in a married woman, H.H.J., 37 years of age, who suffered from severe dyspepsia for some years before the birth of her first child, now eleven years old. Since that event she had had very little trouble in this respect, and, indeed, was considered by herself and her friends to be a woman of particularly robust health.

On August 6th of this year she was somewhat out of sorts, and, attributing her sensations to a bilious attack, she restricted her diet and took a saline aperient. She was quite well next day, and until August 14th, led her usually busy and active life, played tennis, visited her friends, and forgot all about her indisposition.

On this date, while shopping in town, she again experienced sensations of abdominal discomfort and also complained of slight transient giddiness. The same symptom recurred on August 15th.

When seeing her for the first time, I considered the condition to be a simple digestive derangement, and prescribing accordingly, I fortunately advised her to remain in bed. I saw her again on the 16th and unfortunately told her husband that she would be well in a few days. Within an hour sudden and severe hæmatemesis occurred, and on my arrival ten minutes later, I found her blanched and pulseless, complaining of abdominal discomfort and distension, but not of pain.

Dr. Leslie had also been telephoned for, and on his arrival I left to make arrangements for operative interference should this be necessary. During my absence there was further hæmatemesis, followed by collapse so profound and protracted as to momentarily lead Dr. Leslie to believe that she had actually died. When Dr. Ritchie Thompson saw her she had recovered from the immediate effects of the hæmorrhage, and after consultation he suggested the subcutaneous injection of gelatin. We could not, however, obtain any that night, and thereupon substituted two tubes of anti-diphtheritic serum. During the night, and until noon of the following day, the 17th, she felt sick but only occasionally vomited a small quantity of altered blood; 100 c.c. of sterilised gelatin were injected subcutaneously on this day.

She improved slowly up to Sunday, the 21st, when there was a rise in the pulse-rate, and next morning the stools gave evidence of further hæmorrhage. On the 23rd, Doctors Thompson and Leslie again saw her with me in consultation, at which it was decided, in view of the continuation of leakage, and of the possibilty of the occurrence of another large hæmorrhage, to recommend operative measures. Accordingly, on August 24th, she was removed from her house to the Lady Dudley Nursing Home, and on August 26th while there was still evidence of slight bleeding, posterior gastro-jejunostomy was performed. Her condition did not justify a search for a lesion, but on such portions of the stomach, pylorus and duodenum as were easily accessible, no evidence of ulceration could be detected. Her appendix also appeared to be normal to the touch.

Though the operation was attended by much less shock than was anticipated, and though all hæmorrhage ceased when it was completed, her condition for the next fortnight caused us the gravest anxiety. On the eighth day after operation, when the stitches were removed, the wound seemed to be healed, but next morning the dressing was saturated with clear serous fluid, and on removing it the edges of the lower half of the incision (I had fortunately left the middle stitch) were found gaping widely apart, and one could see the viscera to be smooth and glistening, yet almost as colourless as her skin; Dr. Ritchie Thompson, who was present, aptly compared their appearance to that observed at a *post-mortem* examination some hours after death.

The excessive loss of blood, while thus responsible for the suspension of the local processes of repair in the wound, produced at the same time the far more serious general effects of insomnia, restlessness, and disordered cerebral action. She was alternately either excitable or melancholic, with intervals of great physical depression during which her pulse, always feeble and quick, could barely be detected at the wrist. Her temperature, which from the date of the first hæmorrhage had been above normal, continued to fluctuate, after operation, between 99° and 101°, and naturally the possibility of sepsis suggested itself; but Dr. Walter Adam, of Randfontein, who saw her at this time, attributed the mental state to cerebral anæmia, and we, therefore, felt justified in ascribing the rise of temperature to intestinal auto-infection, following diminished resistance.

We had previously tried morphia, veronal, trional and bromide for her sleeplessness, but without effect. Dr. Adam suggested a mixture of potassium bromide, chloral hydrate anl liq. morphinæ, a combination, I believe, much used in asylum work in similar cases of mental unrest. The first dose acted magically; she slept soundly for six hours, on awakening was rational for the first time for five days, and, though naturally enough there were subsequent relapses, she rapidly, with increasing strength, regained her normal balance. She left the Home a month after operation, still very feeble and weak, but in her right mind and perfectly able to digest her food.

Case V.—The next case is an example of the characteristics of chronic hæmorrhage, probably due to the same condition of duodenal ulceration.

J. G., aged 38, was admitted into the Johannesburg Hospital about six months ago, under the care of Dr. E. T. Hamilton, by whose kind permission I am permitted to use the notes of the case for the purpose of this paper. He had been a permanent victim to chronic dyspepsia and anæmia for the past eight years, and it was only when Dr. Hamilton discovered that his indigestion was of the delayed type, associated with characteristic hunger pain, that he was placed in hospital under careful observation. The day after his admission the sister in charge reported the passage of a semi-solid tarry stool, a condition which persisted until a posterior gastro-jejunostomy was performed, some three weeks later, after which no further hæmorrhage occurred. To-day he is in perfect health, has put on weight and is able to digest his food without trouble of any kind.

Dr. Hamilton could not discover the seat of the lesion, either about the stomach or on the accessible portions of the duodenum.

From these five cases it will be observed that the chief difficulty consists in determining when to adopt active measures. In the first instance, when hæmorrhage occurs suddenly and in quantity, we are immediately faced with the problem of deciding whether we should operate at once or whether we could not with safety await the more favourable opportunity that a possible spontaneous cessation of the bleeding would present. On the other hand, in the event of an acute hæmorrhage ceasing spontaneously, and, as in Case III, should there be subsequently no return of the other symptoms of duodenal ulceration, can we with safety tell our patients that operative interference is no longer necessary?

For this reason and also because the control of hæmorrhage, whatever the cause may be, is one of the first duties of the general practitioner, I have ventured to introduce this subject in order to obtain the opinion as well as the experience of Congress.

IV.—INFECTIVE DISEASES OF THE GALL BLADDER AND BILE PASSAGES.

By A. W. Sanders, M.D. (Lond.), F.R.C.S. (Eng.), Pretoria.

It is not my intention to enter upon any exhaustive description of diseases due to bacillary infections of the gall bladder and bile passages, but merely to make a few remarks which shall serve as an introduction to the notes of a case which I believe to have been of this nature, but which perhaps ought to be differently regarded.

Many investigators have shown that micro-organisms are by no means rarely present in the gall bladder in the course of many acute infections, and the number of different organisms that have been found there is considerable, *i.e.*, B. typhosus, the influenza bacillus, pneumococci, staphylococci, streptococci, B. coli communis, cholera bacillus. Whether the presence of micro-organisms in the gall bladder in general hæmic infection is to be regarded as evidence of disease in that organ, or whether it may not be that such organisms are in process of elimination, is, I think, not certain, but I will make some reference to this point later.

The diseases in which infection of the gall bladder and bile passages are known to occur are divided by Rolleston into two classes: (1) General hæmic infection, and (2) diseases of the alimentary canal, in which infection may reach the gall ducts by way of the portal blood-stream or by direct infection along the larger ducts. In Group 1, are septicæmia, pyæmia, influenza, and pneumono-septicæmia; in Group 2, enteric fever, cholera, appendicitis, influenza, and gastro-intestinal disorders in which the contents of the gall bladder become virulent.

I do not propose to do more than mention the fact that acute cholecystitis may occur in any of the diseases mentioned in Group 1, the micro-organisms apparently reaching the bile passages by way of the hepatic artery. Sherrington proved that organisms in the blood-vessels cannot penetrate healthy liver tissue, unless the cells first be injured by the toxin the bacilli produce.

Group 2 is of more general interest to us, especially as intestinal disorders are so common in South Africa. Typhoidal cholecystitis is, however, not very common—at any rate, it is not often so severe as to attract definite attention or demand treatment. It is said that it may occur long after the attack of fever is past. I happen to have seen one case—that of a young man, aged 21, who had acute cholecystitis occurring during enteric fever.

So acute was the onset that I thought perforation might have taken place, but the occurrence of rigor and definite local tenderness with high fever led to a correct diagnosis, and operation showed a gall bladder full of small stones. The presence of stones must certainly predispose to infection in typhoidal or other forms of cholecystitis.

By what channel the typhoid bacillus reaches the gall bladder is uncertain. Direct extension along the common bile duct by the portal venous system, or by the hepatic artery, are all possible means. As I have said, vascular infection must be preceded by some injury to the liver cells by the toxin of the disease. Whether direct extension of the infection from the bowel up the common bile duct can occur is doubtful, for it has been shown by Cushing and Livingwood that in the normal state the duodenal contents are often sterile; but in a disease such as enteric fever, which is so widespread in its manifestations, one cannot doubt the possibility.

One point of much interest in typhoidal infection is that the typhoid bacillus may be found in the gall bladder long after all symptoms of the disease are past, and as they are constantly discharged with the bile the person so affected must be a constant source of infection.

Probably in a majority of cases of cholecystitis the colon bacillus is to be found, certainly in most of the cases in which gall stones are present and appear to be the exciting cause of the inflammation. It must, however, be remembered that cholecystitis due to enteric or coli infection has been shown to be followed by the formation of gall stones. By far the majority of cases of cholecystitis that come to our notice are associated and no doubt largely due to the presence of gall stones.

Now in all the classes of cases which I have given in this brief survey there is at work either a generalised infection, some local lesion of the intestinal tract, or an obvious source of irritation, such as gall stones in the sac, but what I would like to enquire is whether inflammation of the gall bladder and bile ducts can occur as a disease *per se*, unaccompanied, at any rate, by an obvious source of infection. I am able to find descriptions of acute affections of the gall bladder and ducts in which neither stones nor other obvious source of irritation have been present, but I cannot find any description which tallies with the following case.

A man, aged 40, was taken ill, January 8th. He felt poorly while watching some sports, went home and to bed. I saw him two days later. He complained of headache, a feeling of abdominal discomfort, some nausea. His temperature was 101°. I was unable to elicit any definite abdominal pain or tenderness. During a week little change took place in his symptoms. His bowels acted with aperients, the motions being of ordinary character. He had pain indefinitely referred to the right hypochondriac region. No definite tenderness could be elicited. His temperature varied from almost normal to 102°, fluctuating daily. Suspecting typhoid, I had a Widal examination made, but with negative result. On the 22nd he was taken to a nursing home; the following day he had the first of an almost daily succession of rigors, his temperature rising to 103°. His history after this can be discussed briefly. Almost daily rigors followed by sweating; gradual emaciation and anæmia; no pain except an indefinite abdominal discomfort, sometimes called indigestion; some pain generally referred to right hypochondrium, but nothing acute; no marked abdominal tenderness, but generally some tenderness in right hypochondrium. He was cheerful and hopeful. On March 6th he became jaundiced,—not intensely so, but quite well marked; this continued until the end of his illness, gradually disappearing during the latter weeks.

I had slides of his blood examined for malaria, with negative results; specimens of blood removed for cultural purposes were also negative. The tubercular ophthalmic reaction was tried, with negative results. Quinine, either by mouth or hypodermically, had no effect. I tried polyvalent serum, purely empirically, but with no result. Soamin injection appeared for a time to be beneficial, but only for a time.

Dr. Gairdiner kindly saw the patient with me on more than one occasion, but we could come to no definite conclusion as to the nature of his illness. There was some increase of liver dulness in an upward direction, but no other physical sign. A survey of all symptoms made me decide to explore the liver, thinking there might possibly be an abscess. On March 27th, having punctured with negative result, we opened the abdomen. The liver was not enlarged. There were fairly firm adhesions between it and the diaphragm. Separating these, I could palpate the liver over a fairly large area, and could determine no variation in its consistence. There was nothing suggestive of either single or multiple abscess. The gall bladder wall appeared somewhat thickened, and feeling that there was a possibility of trouble there, I opened it. There were no stones; the fluid it contained was bile, rather thick but not obviously abnormal. I put in a tube and drained the gall bladder. No definite improvement resulted from the operation. For a few days the temperature was lower, and then began signs of high fever again. There was a very free discharge of bile from the tube, with some muco-pus. I sent some of the latter to Johannesburg Laboratory, and Dr. Mitchell kindly made a vaccine for me, having isolated a coliform bacillus from this fluid. Four doses of this were given, on April 13th, 15th, 19th and 22nd. The doses of vaccine, judging by the temperature, would appear to have been followed by distinct reaction, but it is difficult in such a condition of irregular and remittent pyrexia to be certain on this point. After the fourth dose the patient's condition seemed worse, so I stopped it, but the exacerbation was followed by a definite decline, and one week after the last dose the temperature reached normal and the disease came abruptly to an end. Convalescence was uninterrupted, the anæmia, jaundice and emaciation gradually disappearing, and now the patient is back at work and apparently in good health.

What was the real nature of this disease is, I think, still a matter of doubt. I cannot but think that it was an infective cholecystitis and cholangitis. Whether the vaccine did cure is also, I think, of some uncertainty, but certainly its use appeared to bring to an end a disease otherwise steadily progressive and quite unchecked by any other mode of treatment.

V.—NOTES ON A CASE OF DELAYED CHLOROFORM POISONING.

By J. W. C. Macpherson, M.B., B.Ch., (Ireland), Stellenbosch.

The patient, a strong, powerfully-built and apparently healthy Cape boy, 26 years of age, got several of his fingers crushed while shunting at 4.30 p.m. on June 24th last. Family history good—father and mother and five brothers and sisters all alive and healthy, and his only illness was a slight attack of jaundice about three years ago.

He was reputed to be a heavy beer-drinker. Owing to some mismanagement, he was not brought to me until 7 p.m., when I found him to be shivering with cold and with a small feeble pulse.

The injured fingers were bleeding freely and he informed me that he had been losing blood all the time since the accident. The hæmorrhage was apparently due to the fact that a handkerchief had been tied round the wrist just tight enough to occlude the veins, but not the arteries. On removal of the ligature the bleeding stopped instantly.

He was put to bed, drank a little hot tea, had hot bottles applied, and as he complained of much pain in the hand, ¼ grain morphia was given hypodermically. He slept all night. Temperature 97.4°.

At 10.30 next morning one finger was removed and other two stitched up. There was no bleeding to speak of during this little operation. (I may state here that the stitches were removed on the fifth day, and all the wounds healed by first intention.)

Chloroform by the open method was administered, and we remarked on the large quantity required to produce even a slight degree of anæsthesia—in fact, he was never properly under; if the operation had not been a trivial one and a matter of minutes, we could not have gone on. This we naturally attributed to his intemperate habits. He was slightly squeamish in the evening, but did not vomit; slept all night.

On the second day he started vomiting early in the morning, and vomited all day at intervals. His eyes and skin were noticed to be markedly jaundiced, and he complained of acute pain over the hepatic and epigastric region. (His mother states that she noticed he was slightly jaundiced three days before the accident.) The liver was found to be tender and distinctly enlarged. He slept all night. Temperature 99°. Bowels moved naturally, stools very pale.

The third day he only vomited once. Urine very dark, containing bile but no albumen. A subconjunctival hæmorrhage appeared in the left eye. Jaundice still prominent. Liver distinctly large, pain slightly less. No motion of bowels.

Fourth day: Vomited once and spat up some blood; small blue patches on tongue. No motion.

Fifth day: Vomited once. Bowels moved well after dose of calomel; motion pale. Temperature 99.8°.

Sixth day: No vomiting. Bowels moved several times; each motion accompanied with somewhat free hæmorrhage.

Seventh day: Bowels moved twice; each motion accompanied with large blood clots. Jaundice decreasing. Pain gone. Liver extended about three finger-breadths below cartilages.

Eighth day: No vomiting. Motion without blood. Seemed better. Urine almost clear. At 2 a.m. smart epistaxis for twenty minutes; otherwise slept well.

Ninth day: Smart epistaxis, lasting four hours, and only stopped by plugging. At 8 p.m. restless and then drowsy, with noisy and sighing respirations; pulse 84, weak.

Tenth day: Drowsy and refusing nourishment (which all along he had taken well). About midday he became unconscious, and died at 8 in the evening. Up to the tenth day he had been rational and cheerful. The temperature rose to 99° two days after the operation, and with the exception of rising to 99.8° on the fifth day, was normal throughout. No sweet smell was noticed in the breath. Urine was not tested for acetone.

Post mortem the heart seemed healthy. There were several small hæmorrhages in the lungs. The kidneys were enlarged, and there was considerable extravasation of blood into the peri-renal tissue on both sides. The large intestine and parts of the small were much engorged and of a blue-black colour. The liver was enlarged evenly, greasy to the touch, and pale in colour; there were no bands, scars or cicatrices; weight 75 ounces. No gall-stones. Mucous membrane of stomach somewhat congested. Blood very fluid.

We have here a diseased liver either from syphilis or alcohol or both: profuse initial hæmorrhage with shock, followed by a large dose of chloroform. May this not have been "acute hypertrophy of the liver"?

Quain mentions "acute yellow atrophy of the liver" where the liver is found enlarged after death.

Report of Examination of Specimen of Liver, by Dr. G. W. Robertson.—Sections cut, after imbedding in celloidin, revealed a cirrhotic condition, which did not conform to any ordinary recognised type of cirrhosis. There was abolition of lobular arrangement, and in no place can any trace of a vena centralis be detected. Fibrous proliferation was very irregular. Frozen sections shewed a good deal of fat when they were stained with Sudan III. The appearances suggest diffuse gummatous hepatitis, but the arrangement of the fat is quite in keeping with "delayed chloroform poisoning" as the immediate cause of death.

VI.—SOME FACTORS WHICH GO TO MAKE OR MAR THE SUCCESS OF SURGICAL OPERATIONS.

By G. A. Casalis, M.B., C.M. (Edin.), Mowbray.

In commenting upon the work of surgeons who are successful the remark is frequently made that they succeed where others fail because "they are lucky." Thus the results of operative work which have been unusually brilliant, instead of being attributed to superior knowledge and skill or more especially to a careful and consistent technique, are too often credited to good fortune. Now, there is no such thing as "good luck" or "bad luck" in surgery unless it be "good luck" for one surgeon to know more, to be more conscientious, and to be more willing to work and keep himself abreast with the times than some of his less progressive and less successful confrères.

If one takes the trouble to enquire why some men are supposed to be "lucky," it will generally be found that the success of their work is the logical sequence of a definite relation between cause, as exemplified by knowledge, judgment and technical skill, on the one hand, and effect, as shown by the character of the results obtained on the other. As a matter of fact it is no fiction to say of modern surgical technique that it has reached such a state of perfection that if followed out in its minutest details it works out with almost mathematical accuracy, or, as is so well expressed by an American authority, "if kept close to the line of highest efficiency it wins practically every time." To speak of "good luck" or "bad luck" is, therefore, for the enlightened operator, the negation of progress; it is to deliberately shut his eyes in one case to what has brought him success, and in the other to what, if he will only take the trouble to investigate, would easily explain his failures.

Leaving aside the personal factor, which is a quantity difficult to estimate, and the question of knowledge and experience, certainly not possessed equally by all, if I were to be asked, What are the main factors in surgical work which would favour success? I should classify them in the ratio of their importance in the following manner:—1st, A rigid and close attention to the observance of aseptic rules; 2nd, the careful treatment and manipulation of tissues; 3rd, rapidity of operative work.

I. A Rigid Observance of Aseptic Rules.

It is a trite saying that modern surgery is mostly made up of attention to details. Successful surgery does really depend, and indeed, as we go on, seems more and more to depend, to a large extent upon a careful and conscientious attention to aseptic and other technical details.

This is exemplified by men who, although indifferent, even inferior, surgeons, score success where others more brilliant and experienced fail, simply because the former are truly aseptic in their methods while the latter are only indifferently so.

It has been said by a bacteriologist that surgeons "strain at gnats and swallow camels." This is true, and more so of the flashy and rapid operator than of his plodding and painstaking confrère. I have seen men who, having taken the most minute precautions to insure sterilization of their hands, of their instruments and of the field of operation, in the excitement of action take hold of a non-sterilized article on an adjoining table, or even readjust their spectacles! I know of one who becomes so absorbed in his work as to mechanically and quite unconsciously pull out his handkerchief and blow his nose, then go on manipulating his instruments as if nothing had happened. But setting aside these flagrant examples of non-asepis, other blunders, just as serious, are frequently made during the course of the operation itself, even by those whom one would least expect to make them. Some time back I saw an able and clever surgeon enucleate the prostate by Freyer's method, during the course of which he had occasion to introduce one finger in the rectum so as to facilitate the enucleation. Having done so, he quite unconsciously afterwards, without having washed his hands, introduced the same finger in the bladder wound while fixing his drainage tubes!

Again, how many surgeons change their instruments after sectioning, say, an appendix, a loop of bowel, the stomach in gastrectomy, or the gall bladder in cholecystotomy? Not all think of doing so, and repeatedly the same scissors are employed afterwards to cut ligatures or for other purposes.

Of course, there are some men, fortunately now getting fewer, who laugh at all these precautions. They argue that the human body is not a test tube in the pathological laboratory, and is capable of some resistance to the attacks of a few microbes. This is quite true, and it is most fortunate for the operating surgeon that it is, for even with the most refined technique one cannot hope to come up to the standard of a bacteriological laboratory; but no one will question the advisability of setting forth such an aim. Where absolute sterility is beyond access it is always of practical service to get as near it as possible and not to argue that because a case is septic to begin with, it is of no use to prevent the entrance of any other germs. Surely, as pointed out by E. Harrison, if there is already infection the patient will probably have as much to deal with as he can safely negotiate, and it is unfair to him to add even a few microbes which may, and most probably will, be of a different species, and so put more work on an already overburdened economy.

Some twenty-five years ago Lord Lister, when introducing his "antiseptic" method, was at the time so impressed with the air as a source of wound infection that he endeavoured to guard against it by operating in an atmosphere rendered antiseptic by a carbolic spray.

This we have now rightly disregarded, as it is known that infection rarely occurs from the air, but nearly always from what is introduced into the wounds by the hands of the surgeon, his assistants or his instruments.

The pendulum has, perhaps, swung too much in the opposite direction, and it seems clear, if we are to believe the very convincing experiments made by Mendes de Leon, and Flügge and his co-workers, that we shall have to pay more attention to the air of our operating rooms. Thus Flügge has shown that in talking, coughing or sneezing, germ-laden droplets or bubbles of sputum are expelled and may float about for some time. The distance to which the droplets may be carried depends on the expelling force, the air currents and the size of the droplets. Tuberculous patients rarely throw their droplets further than 1.5 metres, because of their size. After rinsing the mouth with "prodigiosus" suspensions, Königer found that droplets might be carried as far as 12.4 metres. At present droplet infection is regarded as of more importance in spreading disease than dust infection.

"The specific agents of influenza, pneumonia, diphtheria, whooping cough, measles, scarlet fever, small pox, tuberculosis and the bacteria of purulent infection are conveyed freely in this way."—(*Keen's Surgery*, 1906.)

Mendes de Leon conducted a series of experiments in order to determine how far it is possible to infect wounds by talking over them. He prepared a specially sterilized receptacle into which he talked for an average length of time. In not a single instance did he fail to develop a number of pus-producing organisms, and the virulence of his germs was proved by injections into sixty-one animals. He then constructed a mask to fit the face, when he then, after repeated experiments, proved that no infection was produced.

It thus appears to be of real importance to prohibit talking in the theatre during the progress of an operation, and the operator and his assistant should wear masks fitting over the nose and mouth. One cannot over-estimate the value of a thoroughly competent theatre sister, for on her depends the task of carrying out all the aseptic details, from the sterilizing of the dressings and coats to the final preparation for the operation and that of the patient. It is evident that any failure on her part may prove disastrous. Thus, to note only one factor often overlooked in the sterilization process, too close packing of the swabs in the autoclave will nullify the otherwise most careful sterilization. Reliance on germicides is often also somewhat of a frail reed, and although, as pointed out by Moynihan, the washing out of basins and other appliances in strong antiseptic solutions may be soothing to the conscience of the surgeon or of the nurse, such precautions do not much affect the power of procreation of a pyogenic organism. Prolonged boiling wherever possible is necessary.

On the multiple duties entrusted to our assistants, however, I need not amplify, as every operator who knows that he is surrounded by a well trained staff on whom he can confidently rely is indeed a happy man. For as Senn once put it, " It is the trained conscientious nurse of to-day who fights more than one-half of the battle and who, as a rule, receives so little for her work." It is only unfortunate that such highly trained nurses are so rare, and indeed in this country are practically unobtainable! Certainly one of the most frequent, as well as annoying, of the minor accidents of surgery consists in the occurrence of stitch abscesses. These are usually ascribed, and occasionally with truth, to the use of imperfectly sterilized material. It is, however, unfair to attribute them always, or even commonly, to this cause. After a careful investigation of the whole subject, I am convinced that the great source of infection lies in the impossibility of satisfactorily sterilizing the patient's skin. Consequently the ligature, whatever its nature, carries with it some of the organisms still lying in the supersubcuticular tissues and deposits them in the track of the suture. There may, of course, be other causes. One of these, for instance, is the handling of the catgut with ungloved hands while preparing sutures. Thus it is a common thing to see a surgeon or a sister, before threading the needle, straighten the folds of the catgut, or twist it between the finger and the thumb. This is quite sufficient to infect the ligature, as no method of sterilization of the hands will entirely rid them of micro-organisms. The great secret of eliminating this source of danger is to wear rubber gloves and to handle the ligature as little as possible, but with the patient's skin the problem becomes more arduous and is practically insoluble.

In fact the most rigid care may be given to the sutures, ligatures, instruments, the preparation of the patient and the aseptic technique of the operator and his assistants and yet all this be followed a few days after operation by the appearance of that *bête noire* of all surgeons—pus. After a good many years of constant experiment with the old method of scrubs and compresses, I have come to the conclusion that they are unscientific and unreliable. Our guiding principle should be to avoid softening the skin with consequent detachment of epithelium, which is exactly what the wet compresses does; it is better to harden, to " tan," the epithelial layer and to constringe the glandular orifices. Finally, the hardened surface should be rendered antiseptic.

T. Lionel Stretton recommends the use of a solution of iodine consisting of one part of liq. iodi. fort. (B.P.) and three parts of spirit (equal parts of methylated spirit and distilled water): while Von Herff's method consists of using acetone and combining iodine with it. We prepare our patients in the following manner. The day preceding the operation the abdominal skin, after thorough disinfection, is rubbed for several minutes with a mixture of equal parts of acetone and spirits, and then covered with an aseptic dry compress. An hour before operation the skin is again painted with a three per cent. solution of iodine in acetone and allowed to dry. The same process can be repeated just after the termination of the operation and the solution painted over the sutures. The results obtained by this method are excellent and present at the same time many points of advantage. 1st, It is an efficient method of skin sterilization. 2nd, It is quickly and easily applied. 3rd, It saves the patient the suffering of a preparation very lengthy and very disagreeable. 4th, It saves an enormous amount of labour on the part of assistants and nurses and is less costly. 5th, It can be used in emergency cases when preparation by the usual method is impossible. 6th, Finally, and not least, it leaves the skin firm and hard, with an absolutely aseptic surface, which is less likely, especially should Michell's clips be used, to lead to any secondary infection.

Sterilization of the patient's skin is, however, of little avail if, during the course of an operation, care be not taken to preserve, by means of aseptic towels, its surface from all other sources of contamination. Special forceps, such as those of Houzel or Lane, have been constructed for this purpose and successfully pin the wound edges to the sterile cloths employed. Thus the skin surface does not become macerated by the blood or other fluids escaping from the wound or abdomen, or dripping from the hands of the surgeon and his assistant, when antiseptics are used, and it keeps its hard and glossy appearance to the end of the operation. The wound edges require just as much protection as the general peritoneal cavity and the viscera. The pus from inflamed fallopian tubes, the bile from an infected gall bladder or fæcal matter escaping from the bowels, if brought into the slightest contact with the abdominal wound, is sure to result in suppuration and later in a weakened scar and possibly a ventral hernia. Too great care cannot therefore be exercised in the proper protection of the skin surface and wound edges.

II. The Careful Treatment and Manipulation of Tissues while Operating.

Like all great principles, once they become recognised and established, that of asepsis has had the effect of overshadowing other important principles by concentrating the minds of surgeons on one particular point and consequently inducing neglect of others. There is little doubt that the extraordinary safety of modern surgical procedures, brought about by our control of microbic action, has had a tendency to obscure the great importance, when operating, of conserving tissue vitality to the utmost.

Thus the frequency with which leading surgeons find it necessary to caution others less experienced or more careless against the unnecessary handling of the abdominal contents in the course of laparotomies certainly suggests a deplorable tendency in that direction. That such rough usage of the bowels and peritoneum plays a great part in the occurrence of post-operative adhesions, leading, for instance, to the formation of kinks or to omental fixations, no one with any experience in abdominal surgery can deny. In fact, there seems to be little doubt that many of the operative results which, though apparently successful at first, have not come up to expectations, are frequently due to nothing else but to disturbance in position and in function of the different viscera in the abdomen.

I have lately seen in consultation a lady who, after two serious abdominal operations, is still partly laid up, because her omentum and probably her cæcum are adherent to a wide and certainly too extensive a cicatrix. Again, it is not unusual to have patients complain of a more or less obstinate constipation, which did not exist previous to an abdominal operation. In many of these cases the diagnosis of dilation of the colon, want of peristaltic action or general asthenia of the abdominal contents is made, whereas if the truth were known it would be found that the trouble depends mostly on a bad fixation of the omentum with traction on some part of the big bowel. In others, again, a faulty position of the sigmoid or adhesions of the small intestines at the bottom of Douglas' pouch, after hysterectomies, for instance, is responsible for the constipation. Thus I recall a case where a woman who had been operated upon for an ovarian growth, came twelve months afterwards to see me, suffering apparently from all the symptoms of gastroptosis, to which was added tympanites and constipation. A careful examination failed to detect gastric incompetence, but she had an adherent retrodeviation and tubo-ovarian disease on the right side. The operation relieved the utero-ovarian condition and also that of the sigmoid which, together with the omentum, were found firmly adherent to the upper surface of the uterus. Since then the gastric symptoms, as well as the constipation and tympanites, have entirely disappeared.

On May 5th, 1910, a young married woman, returning from England, consulted me for symptoms very suggestive of gastric disease, complicated by gall stones. There was a past history of gastric ulcer, and she had undergone, only a year before, a serious pelvic operation, in Johannesburg, for an infective condition of the tubes and ovaries, which unfortunately had completely unsexed her. While in England she had been seen by no less an authority than Mayo Robson, who had also diagnosed dilation of the stomach and gall stones. He advised operation. I did the same, and she was admitted to my Home and operated upon. The abdominal incision was the usual one employed by Robson. To my surprise, I found both the gall bladder and biliary ducts perfectly healthy. I then opened the gastro-colic omentum and inspected the whole contents of the lesser sac and carefully palpated both the anterior and posterior gastric walls. Everything was normal; there was no trace of a past ulcer, either of the stomach or duodenum. Inspection of the lower portion of the abdomen revealed, however, extensive omental adhesions right down to the pelvis and the old cicatrix. These were released and the abdomen closed. Recovery has been uneventful and the patient seems to have regained her full digestive functions. The omental adhesions appear, therefore, to have been the only cause of the symptoms complained of, and by dragging down both the transverse colon and stomach produced probably a kink at the pyloric and duodenal band, thus creating a condition not unusual in gastric ulcer and gall stone disease.

The prevention of adhesions by all the means we possess appears, therefore, to be of paramount importance. Among these, the careful protection of the abdominal contents by aseptic compresses while operating is essential, and the intestinal coils should be at all times exposed as little as possible and handled with the greatest care and gentleness. It is also of importance that no antiseptic solution should be permitted to reach the peritoneum. There is abundant experimental evidence to show that the delicate peritoneal surface can be seriously damaged by these solutions and its power of absorption thus be considerably lessened. The majority of modern abdominal surgeons employ a sterilized salt solution, which is just as effective for rinsing of instruments, swabs and ligatures when rubber gloves are worn.

Another and no less important precaution is the careful use of swabs and compresses intra-abdominally. The constant swabbing and sponging indulged in by some surgeons is not only often unnecessary but may do infinite damage. A little serum or blood in the abdominal cavity is of little importance; it may possibly even be of some advantage, but certainly does infinitely less harm than the partial denudation of the peritoneum of its precious endothelial lining. The best safeguard against against post-operative adhesions, however, consists in peritonising any raw surfaces or solutions of continuity on the peritoneum left over after the excisions of organs or growths; further in dispensing, wherever practicable, with drainage tubes or gauze plugs and also in learning to work through the smallest incision possible.

Of late the theory, now somewhat extensively put into practice, has been advanced that early rising and a shortened convalescence from laparotomies is of material advantage and prevents peritoneal adhesions. Without going to the extremes advocated by some, there is little doubt that early movement is beneficial in many abdominal cases, and allowing, in some favourable and well-chosen case, the patient to get up about the eighth or tenth day has, in my experience, been followed by no ill-effects. The convalescence appears, on the contrary, to be hastened. But early rising is certainly not applicable to all and every section case. Much depends on the nature of the operative case, whether, for instance, there has been inflammation or not. When there is, Nature's precepts teach us by the presence of pain that rest is beneficial, and nothing, at least in uterine cases, is of more value than two to three weeks in bed. As regards drainage, everyone is agreed that it should be dispensed with whenever possible. The absorptive faculty of an unimpaired peritoneum is a valuable asset not to be lightly dispensed with. Big incisions are frequently necessary, but it is safe to phophesy that the future belongs to the operator who will work through the smallest hole he can possibly make in his patient and perform his work quickly and well.

Of course the smallness of the incision should not be such as to interfere with efficiency when it is necessary for the surgeon to see well; in the surgery of the gall bladder or stomach, or in the total excision of the uterus, a large incision is by far the wisest policy. In fact, the safety of such operations depends on the preliminary wide exposure of the parts to be operated upon, but there is no doubt that such incisions favour, infinitely more than the small ones, post-operative adhesions.

But it is not only the grosser forms of what an American writer calls "the mauling of tissues" that we wish to protest against; we refer also to the more common and apparently insignificant forms which are nevertheless quite as potent for evil. In operating one should never forget that we are dealing with living material, and that every point or line of compression results in some degree of tissue necrosis, in the somatic death of some cellular entities, and that such necrotic tissues form an excellent germinating ground for any micro-organisms which may find access there. If the absolute exclusion of pathogenic germs could be attained, this would not perhaps greatly matter, but all asepsis, however carefully sought, can only be relative; hence the desirability, the duty even, of limiting the "mauling" of tissues by forceps, ligature or suture to the least degree possible.

As regards the use of forci-pressure forceps, there is little doubt that now-a-days they are employed in and out of season, and frequently with the most deleterious effect possible on the tissues to which they are applied. Originally they were intended to constrict only the mouth of bleeding blood-vessels, but it is now not uncommon to see a row of them garnishing the skin edge of an abdominal incision. The fascia receives the same treatment, so does the peritoneum, and all the structures are forcibly constricted by instruments with serrated edges, and so tightly applied as to seriously endanger the vitality of the constricted portion. The proper function of forci-pressure forceps is not to act as retractors but simply to control hæmorrhage. If retraction there must be, a blunt instrument, fashioned *ad hoc,* is the correct one to use, and certainly not one whose force is such that it increases to a considerable extent tissue necrosis. Further, retraction of the wound edges is frequently unnecessary, if not harmful, but should it be imperative and ordinary retractors not be at hand, the employment of forceps constructed on the Lane principle is safer.

Generally speaking, it may be said that the majority of ordinarily constructed forceps are altogether too big and clumsy, with a grip too large and with too extensively serrated a surface. I have completely given up such instruments and prefer to employ fine-pointed Kocher forceps with the well known rat-toothed nose. For intestinal work I use the finest obtainable and with edges not serrated. For clamping ligaments, the vagina or other large structures, light clamps, constructed on the Kocher principle to prevent their slipping, are quite sufficient for all practical purposes and there is no reason whatever to employ the heavy articles generally supplied by instrument makers.

As regards ligatures I have now for some years employed catgut exclusively. I think the evolution of modern surgery points to an increasing tendency on the part of surgeons to use absorbable material for buried ligatures and sutures. Apart from the at one time unsatisfactory nature of catgut ligatures, the conservative tendency to cling to silk was due, I believe, to the widespread belief that the occlusion of an artery or vein by ligatures depended more on the continued presence and pressure of such a ligature than to the unseen plastic changes taking place in the vessel after its application. Hence the tightness with which such ligatures were, and are still, being applied. From a surgical and practical standpoint one feels sure that more ligatures are tied too tightly than too loosely. Certainly nothing is more ludicrous in an operating room than to witness a sturdy operator attempt to tie a small or, perchance, a large artery so securely as to break ligature once and again, and then, with language too forcible to quote, condemn the weakness of the material supplied, and congratulate himself on the timely discovery of its brittleness. On most of these occasions it is safe to assume that the ligature is quite sound and sufficiently resistant, but is not a cable and cannot be treated as such. Walter B. Chase (*Journal of A.M.A.*, November 30th, 1907) points out that a very little of the circular pressure caused by the ligature is sufficient to prevent hæmorrhage and that tying too tightly tends to the premature destruction of the vein or artery, and predisposes to the risk of secondary hæmorrhage by cutting its walls. There is little doubt that most of the secondary hæmorrhage following laparotomies or other operations arise from this cause, and that by applying undue pressure to the walls of a vessel the very accident which was hoped would be averted has been induced. Of course much depends also on the material adopted for ligature, as well as on the size of the strand used and the mode of its preparation. Generally speaking, it may be said that the smaller the size and the more quickly it is absorbed, the better it is for all concerned.

The time required for plugging a vessel also depends upon its size and partly upon the plasticity of the blood and the resulting exudate, but if the ligature has severed the inner structure of the artery the physiological closure of the vessel is sure to be accomplished and the time required to effectively seal its mouth against the risk of bleeding is surprisingly short. Most small vessels, if properly tied, will become effectually occluded in a few hours, while a ligature which lasts from three to five days usually gives all the safeguards of one lasting double that time.

There is, of course, *per se,* no objection in many cases to using ligatures which last a longer period, but contingencies which require the use of unabsorbable material are few and should become fewer still. An unabsorbable ligature is ever and always a foreign body and an ever present cause of pain, possibly of sepsis, and finally, of ulceration and suppuration. That such an infection has not always been introduced by the surgeon or his assistants nor been provoked, as often implied, by the imperfectly sterilized nature of the material employed, would appear at times possible. Thus the studies of Behring on latent tuberculosis infection and the clinical experience of many observers point strongly to the probability of pathogenic organisms remaining quiescent for long periods in the body but suddenly entering into activity only when some exciting cause, such as traumatism, has lowered the body resistance.

Thus while there is every reason to believe that under normal conditions the blood and lymph currents are sterile, Wright's recent work on the opsonins has proved that the power of leucocytes to maintain that sterility by phagocytic action is frequently a variable property. Should, therefore, the opsonic index, which may be taken as indicative of bodily resistance, be below normal, as it often is in cases requiring operation, it is reasonable to believe that certain overlooked depôts of pus or other organisms such as occur in otitis media, caseous glands, chronic gonorrhœa or pyorrhœa alveolaris, may pour infective material into the circulation and that this may be swept along to vulnerable points where active growth is taking place. On such counts alone can we account for the isolated abscesses which occur in the hip joint, knee joint, the parotid gland and other parts of the body. In any case, the migration of pathogenic germs from unsuspected areas of chronic suppuration alone satisfactorily explains cases of latent infection which have puzzled many an operator, but it also serves to show that the personal equation of every patient operated upon is not the only important factor to consider in the remote results following an operation; causes of irritation, such as an unabsorbable ligature, have an even more immediate bearing on the possible occurrence of post-operative sepsis.

It would be easy to refer to multitudes of cases in which sinuses, infections, and even mortalities have followed the use of such ligatures. Of absorbable ligatures, sterilized catgut, plain or chrominated, has hitherto been employed and generally fulfils all purposes. It must however be confessed that, although the demonstration by actual scientific tests of the perfect sterilization of catgut has now been amply proved, still it is an object which possesses the most unstable aseptic nature ever known, and there is no surgeon of some experience who, in spite of the most rigorous asepsis, could not quote cases of infection arising from its use.

Perhaps of the many accidents occurring in the course of surgical practice none has provoked more comment or is more distressing than the development of post-operative tetanus. That it is far from uncommon and rather on the increase is well known. Thus I can recall, within seven years, five cases which I have known to occur in surgical practice in this country. When these deplorable and regrettable cases occur, it is customary to blame the ligatures or sutures employed. No doubt in a considerable proportion of cases tetanus does result from this source, but this fact increases rather than diminishes the surgeon's responsibility. Not only is it his duty to select sutures that have been prepared by methods guaranteeing the greatest amount of protection, but, since it is a bacteriological fact that the tetanus spores and bacilli can be destroyed by a temperature of 212° F., he should always take the extra trouble to see that the unbroken tubes which contain his ligatures are boiled for twenty minutes with the instruments, or, better still, placed with the dressings in a high-pressure steam sterilizer for at least half an hour, and under not less than fifteen pounds' pressure to the square inch. Catgut preserved in absolute alcohol in air-tight glass tubes can undergo such a treatment with absolute safety, and is then positively free from any factor capable of giving rise to tetanus or any other form of sepsis. If tetanus then makes its appearance the infection arises from other sources, and as its common habitat is the soil, it is not difficult to understand how easily it might reach the field of operation through the hands of the operator, the nurse or the dressings.

Recent writers have pointed out the difficulty of thoroughly disinfecting the hands, especially when the skin is rough and chapped. In fact while it is possible, as regards sutures, instruments or dressings, to entirely eliminate the danger of sepsis, as regards the hands there is a constantly varying equation in the degree and character of infective material, depending upon the individual himself or his immediate occupation, which no measure of disinfection, including scrubbing in flowing hot water, painting the nails with iodine, or soaking the hands in different antiseptic solutions, can ever completely eliminate. The only safety lies, not only in a definite inflexible routine of hand disinfection, but above all in the wearing of aseptic rubber gloves. It must be remembered that every time a knot is tied with bare fingers organisms are squeezed from the sweat glands and sebaceous follicles of the operator's hands on the ligature, by the force necessitated in the tying, and the danger of infection is intensified by twisting or rolling it in threading needles. The only remedy is to wear rubber gloves, and no surgeon with any regard for his patient's safety or pride in his work can afford to do without them. It is no more a question of *prudence,* it has become one of *necessity* and of *conscience.*

Recognising the many chances of sterile catgut becoming infected before and after its introduction in a wound, some operators have become impressed with the advisability of adding some germicide to its outer surface, and thus rendering it both aseptic and antiseptic, hence the gradually extending practice of employing the so-called iodised and formalised catgut. There is no doubt that this can be accomplished without injury to the gut and without danger of wound irritation. In fact after experience of both methods we have come to the conclusion that the addition of a strong antiseptic like iodine or bichloride of mercury to an aseptic strand of catgut adds greatly to the safety.

Catgut prepared with iodine may also be chrominised and thus rendered less absorbable. Plain iodised catgut ordinarily lasts three to ten days, according to size, but in muscular structures a shorter period; chrominized gut from ten days upwards, according to the degree of its chrominization. It is important to remember in this connection that catgut can be so over-chrominized as to render it unabsorbable, and make it as objectionable as silkworm gut. We can call to mind a case where we had fixed the uterus with chromic gut, and which, to our great annoyance, was followed by a most troublesome fistula, which only closed when the catgut was extracted six months afterwards; it was then practically unimpaired.

As regards suture material, except for intestinal work, it is also advisable to use catgut only, although for the skin I still prefer silkworm gut, or better still, Michell's clips. As a rule it may be said that for buried sutures the tendency of most operators is to employ suture strands too large for easy absorption or to place too many within the wound area, forgetting that the tissue cells and leucocytes surrounding sutures are capable of absorbing only a limited amount of foreign material and that if more is inserted than can safely be taken care of, local irritation is sure to arise, thus establishing conditions very suitable for the growth of organisms. This is especially the case in overtight suturing which often causes strangulation with consequent tissue necrosis. Halstead in his article on "Wound Treatment, with Special Reference to the Value of the Blood Clot in the Management of Dead Spaces" (*Johns Hopkins Hospital Reports,* Vol. 11, No. 5, 1891), wrote: "I believe that the obstruction to the circulation produced by sutures and ligatures is often the immediate cause of suppuration in infected wounds, and that the larger the masses of tissue constricted, and the tighter the strangulation, the greater the danger of suppuration." In brief, he argues that surgeons have hitherto had an unreasonable fear of the presence of blood and fluids in the dead spaces of operation wounds. In subcutaneous traumatisms nature clots these fluids; why should the clotting not be equally harmless in surgical traumatism? The harm that has hitherto been associated with unstained wounds he considers due not to the pressure of the blood clot but rather to sepsis, frequently favoured by tissue constriction due to tightly-drawn deep sutures. Of the many evils resulting from this practice and from that of using too large or too many sutures, mention need only be made of thrombosis and the production of emboli, frequently carrying off a patient when convalescence seemed assured. How many of these cases are to be traced to faulty suture material seems difficult to establish; but I have the distinct impression that the condition known as "phlegmasia alba dolens" was certainly more frequent when silk was used to suture, for instance, the cervical stump in supra-vaginal hysterectomy. It has also been a frequent personal experience that the less catgut I have employed intra-peritoneally the more rapid and even has recovery been in extensive pelvic operations. Thus when catgut sutures and ligatures of known resistance and of appropriate size had been properly adapted to the tissues they were intended to constrict, absorption took place naturally, and with no disturbing element.

But if the strand used had been too resistant, too large, or knotted too heavily, or, worse still, too tightly, the temperature chart invariably indicated what has been called "latent sepsis." In pelvic work, vaginal examination will then frequently reveal an extensive exudation area, due to effusion and leucocytosis—Nature's method of dealing with foreign material; but it is easy to understand how these reparative forces may become overtaxed and thus disaster follow.

Extended investigation and experimentation has developed a great deal of important knowledge in this direction. For instance, it is now known beyond doubt that chrominized catgut which will specifically resist absorption in muscles for ten, twenty, thirty, and forty days, will, in secreting membranes or organs, undergo absorption practically four times faster. Consequently in the kidney, liver, stomach or serous or mucous membranes, rapidly absorbable sutures should never be used. Indeed in intestinal surgery silk is the only safe suture material to employ as catgut is not sufficiently resistant and is sure to give way, with all the inevitable dangers of such a result. On the other hand, in tissues like muscle or skin, it is equally unwise to use sutures too resistant to aborption. Such sutures invariably tend to overtax the tissues and to produce irritation. Again, it should be remembered that where the tension must of necessity be great, as in certain abdominal work, the suture should be large and of adequate resistance. When the tension is slight a suture of small size and fairly rapid absorbability may be used, not only with impunity but often with great advantage.

III. Rapidity of Operative Work.

But if attention to details plays a leading *rôle* in the success or non-success of an operation, there is another factor which is almost as important, although for the last fifty years it has been considerably neglected, namely, rapidity in operative work. This is a quality which in the pre-antiseptic days and before the discovery of chloroform was the quality, *par excellence,* which stamped a great surgeon and made his fortune. To be able to operate with marvellous dexterity, to remove with lightning speed a limb or open a deep-seated abscess before the patient could realise the knife had already done its work, was considered, and rightly so, as the attribute of mastery. But with the advent of anæsthesia and the safety of modern surgical procedures, much of what used to be prized has now been disregarded and is considered only as of secondary importance. Sir Frederic Treves has just written in the *Nineteenth Century* an extremely interesting article with the very whimsical title, "Are We Losing the Use of our Hands?" wherein he points out, with much pathos, that owing to the supremacy of the machine, men have no longer many of the physical qualities which were developed in the stress and strain of savage life.

Thus we are losing the marvellous skill of the hand which was developed by our ancestors. The typewriter destroys the use of fine caligraphy and sewing machines destroy fine sewing. In our own profession, " Surgery, as a pure handicraft, reached a point of perfection, prior to these great changes, to which it does not now attain." This, however, Sir Frederic Treves admits is due not so much to the machine as to the introduction of anæsthetics, which allow the surgeon to take time. It is not an exaggeration to state that, for the majority of men, time in operative work is not only a negligible quantity but it is altogether at a discount. How many observers time their operations? Very few. Indeed, I venture to remark that the man who attempts to work against time, if not actually blamed, is regarded a little as something of what the French call a "*faiseur.*" Has Doyen, for instance, not been stamped a " charlatan " because he had his operations cinematographed so that he should learn not only to operate rapidly and systematically, but above all because an examination of the films revealed to him many movements which, unobserved at the time, prolonged his operations and could with advantage be dispensed with.

" Take your time " used to be the keynote in the teaching of a well-known French surgeon, now dead—*festina lente*—and he used to do so with such success that his operative mortality was double that of any other surgeon of his day. It is still the motto, avowed or not avowed, of many among us. Thus we now frequently witness operations which should be performed, say, within forty minutes—and few major interventions need overstep that time—last one, or even two hours, and still, although in a fair proportion of cases such a protracted operative procedure can now be indulged in with comparative impunity, there is little doubt that in others this deplorable tendency to overstep the limits of resistance in a patient already weakened by disease proves unwise and at times fatal. Take for instance the operation for acute appendicitis or general peritonitis. Is there a class of case where it is more the paramount duty of the surgeon to subordinate everything, yea, even, if need be, the sacred principles of asepsis, to rapidity?

Referring to these cases, Moynihan says: " I do not think that in any abdominal operations ever undertaken by the surgeon the need for speed, combined with aptness, can be greater than in these. Slow operations mean death from shock, imperfect operations mean death from a continuance of the acute inflammation. Perfunctory work and slow work are both out of place." Again, speaking of the successful treatment of cases of infective appendicitis with peritonitis and with products of infection in the peritoneal cavity, an American surgeon well known for his dexterity in abdominal work, says: " Let the patient have the shortest possible period of anæsthesia, the shortest incision through which a surgeon can do his work, and let that work be completed in the most rapid manner compatible with the degree of safe dexterity. Do not stop to do ideal and fanciful work with the stump of the appendix. Get the appendix out in any practical way, so long as it is done quickly, without regard for artistic technique. Pay little attention to separation of adhesions, excepting such separation as may be absolutely necessary for the removal of the appendix or for opening multiple abscess cavities. Pay no attention to pus which gets upon normal peritoneum while you are at work. The peritoneum will care for it better than you can with your wiping and washing. Put in the smallest capillary drain which will suffice to lead products of infection out of the peritoneal cavity. Do all this in five minutes, if possible. Get it in quickly, and get it out quicker. In one word, let this be your watchword: 'Run while pus is running! *Pollice presso!'* Don't stay to kill the patient." Valuable advice, which every surgeon with ambitions to become a laparotomist should read, learn and inwardly digest!

Rapid operative work, in spite of what is said to the contrary, is therefore still an element of success which cannot be overrated. It is a quality which is worth cultivating and should still be as eagerly sought after as in the olden days, when we had no anæsthesia. But by this we do not mean skipping over one's work, neglecting minutiæ, and hurrying over the different operative steps, simply with the desire to get there quickly. The real surgeon is one who, with all the operative details at his finger tips, goes systematically over the ground he has to cover, loses no time, makes no false steps, and, without flourish of trumpets, terminates his work in the shortest time possible. Such an ideal, however, requires qualities not possessed by all. It demands, besides knowledge and judgment, practice, quick decision, pluck, endurance and that special "surgical sense" which so few have but which may sometimes be acquired by long practice and patience.

VII.—A CASE OF CANCER OF THE RECTUM TREATED BY THE ABDOMINO-PERINEAL OPERATION.

By C. C. Elliott, M.D., B.S. (Lond.), Cape Town.

I bring this case forward for report and discussion in the present unsettled state of opinion as to the best route for operation in rectal cancer. As you know, there has been a good deal of discussion on the subject lately, and I think it wise to record cases, whether of success, of partial success, or of failure.

My patient is a man of 63, strong and of sound constitution. For the last twelve months he has suffered from constipation and needed to take aperients, which have given him some colicky pains. He was examined by two practitioners, but they did not find out the cause of his illness. With the motions there was some mucus stained reddish, but no independent discharge of mucus or blood. The motions were loose, owing to the use of aperients. He thought he had piles.

In June, 1910, I examined his rectum and found the edge of a growth high up. Even under an anæsthetic a few days later I could not get my finger high enough up to pass into the lumen of the growth-surrounded gut. But I could feel the mass more distinctly, and it appeared to be fairly movable.

Accordingly, on June 15th, I operated, having had him in hospital a few days previously, in order to wash out the bowel and clear it with purges.

On opening the abdomen I found the growth movable and free from bladder and surrounding structures. It was above the level of the prostate, and about 2½ inches from the sigmoid ending of the colon. No secondary growths were felt above.

I divided the bowel about three inches above the growth, tied the proximal end with silk, enveloped it in gauze tied round it, and laid it outside the abdomen. I then separated the bowel below as far as I could, having similarly occluded the distal end, tied vessels and connections off, and packed the whole divided gut into the pelvis. The narrow male pelvis made this procedure by no means easy. I could separate the growth by this means from attachments. I then placed the patient in the lithotomy position, made a racket-shaped incision round the anus, and dissected out rapidly the whole rectum, not sparing the sphincters. I was then able to get hold of the upper part already cleared, and delivered the whole mass from below. I then inserted a large tube and gauze drain. I then brought out the proximal end of the colon through a separate opening in the left inguinal region and fixed it there, inserting a Paul's tube to drain off fæces that might descend. I finally closed the central abdominal wound in the ordinary way in three layers. The patient's condition remained excellent throughout the operation. There was no shock, and the pulse was 80 when he was put back to bed.

He did very well for a week, then suffered from great weakness and collapse, and appeared to be in great danger. Some suppuration occurred in the muscular layers, between the central and the inguinal incisions, but discharging by the latter. There was also suppuration in the pelvic wound, which proved rather obstinate. He gradually, however, became stronger, and now has a good artificial anus; the perineal wound has healed (October 31st).

The growth proved to be a glandular carcinoma, as Dr. Robertson reported. Behind the original growth is a mass of glands which came away with the whole mass intact. On the surface of the peritoneum covering the rectum in front are some small nodules. The lumen of the gut was narrowed so as to admit only a Spencer Wells forceps.

In this particular case I do not think there would be much difference of opinion as to the best method of attacking the growth. Of course it might have been possible to extirpate it by the sacral route, but it would not have been so satisfactory. It must be remembered that the growth could only with difficulty be reached *per anum* by the examining finger, so that it was impossible to form an idea of its size and nobility. By exploring from above it was possible to make sure of these points, and no harm would have been done if the growth had proved to be inoperable.

But the question is not always so simple, and I proceed to discuss the relative advantages and disadvantages of the different operations proposed in cases of rectal cancer.

There is no doubt that the female pelvis is much easier to work in from above than the male. In my own case I soon appreciated the difficulty of working in a narrow male pelvis. Handley even says (*B.M.J., June* 4*th*, 1910, *page* 1379): "In the male sex I agree with Mr. Cripps in advocating the operation from below. The deep and narrow pelvis of men, and their liability to profound shock from pelvic interference, make the combined operation a prolonged and dangerous one." But nevertheless it is often done in men, and, as Groves remarks (*B.M.J., June* 11*th*, 1910): "I am surprised that

Mr. Handley attaches so much importance to the question of shock in the combined method as applied to male patients. One of the most satisfactory points about the method, as I have seen it, has been the remarkable absence of shock. It has repeatedly been remarked upon by the anæsthetist as being much less than in the simple trans-sacral method. *Re* the case (described in this letter), although the operation lasted for two and a half hours, the patient's pulse was never over 100, and was so good at the end that it was not necessary to give a saline infusion. I attribute the absence of shock to the fact that the tissues are cleanly divided instead of being dragged upon and loaded with heavy clamps." My own case bears this out, for at the end of the operation the patient's pulse was only 80.

One great advantage of the abdominal operation in all cases is the possibility of exploring the extent to which the growth has spread. Sometimes, even though the sacral glands are not involved, glands higher up are, and these could not be detected by the sacral route. Again, the liver may be palpated and secondary growths felt in it. Thus, Groves (*loc. cit.*) instances a case where the growth was freely movable and within easy reach, but a medium incision revealed several metastatic growths in the liver.

As to the amount of rectum that should be removed, opinions vary. Cripps thinks (*B.M.J., August 28th,* 1910) that Handley, in his Hunterian Lectures (*B.M.J., April 16th,* 1910), needlessly advocates a removal of too great a length of rectum in early cases. Handley, by his new method of staining, states that he has demonstrated cancer cells very far from the original growth. He says in one case of his he proved the spread by permeation along the mucous lymphatic plexus of the bowel six inches above the growth. Cripps thinks this is hardly proved, and adduces clinical evidence to show that removal of a much smaller area is often attended by non-recurrence, to which Handley replies that "the incomplete extirpation of a cancer is sometimes followed by a permanent freedom from recurrence, owing to the activity of the natural defensive processes, or the exhaustion of the proliferative powers of the cancer cells themselves," and points out how, in all operations for cancer, the great aim is to remove, as radically as possible, the affected area.

The question as to whether an artificial inguinal anus should be made, or the divided ends of the bowel united, or the divided upper portion be brought down to the anus, is an important one. In low growths, of course, it is possible to remove enough of the growth, and yet to bring down the divided upper end, and even to save the sphincter, and when possible, this may be done. Some, after performing the division abdominally, have gained access also sacrally, and performed a junction of the sigmoid to the lower part of the rectum. This may be successful and desirable in rare instances, but the risks of recurrence seem to me to be greater, owing to the insufficient removal, and there is also the danger of the parts not joining. In most instances it is far preferable to make an artificial inguinal anus, and remove the whole rectum.

I should like to quote an illustrative case of Mr. Handley's (*B.M.J., April 16th, page* 927) abstracted.

"Mrs. G., aged 32, had a typical carcinoma of the rectum, a finger's breadth above the anus. The bowel was mobile, and the os-like stricture just admitted the finger tip. No enlarged glands or secondary deposit could be felt, and the growth had not completely surrounded the bowel. By a combined abdominal and perineal operation, I removed the lowest ten inches of the bowel, including the sphincter and the peri-anal skin. Access to the rectum below was obtained by an incision in the middle line of the posterior vaginal wall. The upper end of the sigmoid was brought out through a separate puncture in the left iliac region, just within the anterior superior spine. Recovery was delayed by cellular suppuration in the pelvis, but the patient is now in excellent health."

In regard to the site of the artificial anus, I followed the advice of Mr. Swayne, of Bristol, who relates an interesting case (*B.M.J., February 12th,* 1910), where he adopted the abdomino-perineal route in a woman aged 56, where the growth was 2½ inches from the anus. He points out that it is undesirable to risk fouling of the large central incision by the necessary escape of fæces. Handley adopted the same plan, and argues in its favour.

Mr. Miles has published (*B.M.J., October 1st,* 1910) a very interesting paper, read at the recent meeting of the B.M.A., in which he details the steps by which he was led to abandon the perineal approach by any method, and to adopt the combined abdomino-perineal. Though employing more and more thorough extirpation by perineal methods, he found all unsatisfactory, and by 1906, having performed 59 operations, perineally, with one death, he found that 54 of these suffered from recurrence, 3 alone remaining well. Consequently he adopted a very thorough combined operation, and has had 26 cases. The immediate mortality is much higher than by the perineal route alone, for he says that 10 died within from twenty-four hours to twelve days after the operation, certainly a high percentage. In 12 of the remainder there is no recurrence, after from a few months to over three and a third years, but naturally time will have to elapse before a final estimate can be given. The extensive extirpation Mr. Miles practises included excision of the coccyx and the levatores ani as far outward as their origin from the pelvic wall, with the fat and fascia between. Of course, he establishes a permanent colostomy opening inguinally, closing the median abdominal wound, and has to re-establish a pelvic floor by suturing peritoneum together, often having to utilise anteriorly the peritoneum dissected from the bladder, or, in the female, the uterus.

I shall not long detain you with statistics, as the later operation has not been done sufficiently often for an adequate number of cases to be collected. Kraske's operation does not give such bad results according to some as according to others. Thus Gant gives 20 per cent. mortality in the hands of twelve well-known surgeons, and permanent cures as under sixteen per cent. Swinford Edwards, on the other hand, has performed the operation seventy-eight times with only four deaths and twenty-two recurrences. This surgeon thinks that it will be long before the abdomino-perineal operation can compete with the sacral one as regards immediate mortality, but my impression is that, when an operation is newly undertaken, the mortality is greater than when the technique is mastered and experience gained, and if there really are advantages in the newer operation, and indeed there appear to be (such as fuller exploration and more complete removal of glands), I think it will become more generally adopted, for after all, we must remember the absolutely fatal character, when left to itself, of the disease we are attempting to deal with.

Discussion.

Dr. Ritchie Thompson said that by the combined operation one was able to diagnose extent of growth and to deal with glands. The cases he had seen had been rapidly fatal. He laid stress on Moynihan's point that one should go higher up than was necessary for removing growth. It is possible to mobilise the colon by dividing the meso-colon right up to the splenic flexure and bring down the transverse colon.

Dr. Casalis remarked that the surgery of these growths was the same as those of malignant growths of the cervix. Vaginal hysterectomy was formerly more common for carcinoma of the cervix, but now the abdominal route was more frequently used, and was easier, safer and better. The infiltration of the peri-rectal tissues was very difficult to localise, especially where there is prolapse of the rectum or vaginal wall.

Dr. Daneel said that the radical combined operation had not been developed sufficiently, but was the operation of the future. From the researches of Handley we now know that metastases extend very high, and this fact goes further to justify such an extensive operation. Up to the present the remote results of the unilateral extirpation of Kraske had been very poor, and if we wanted to improve in this direction we must, as in the case of hysterectomy for cancer, become more radical. The efforts to get a clue to prognosis from the microscopical nature of the tumours had not been very successful.

Dr. Elliott, in reply, said that he preferred the permanent opening to the method mentioned by Dr. Ritchie Thompson.

VIII.—LOCAL ANÆSTHESIA IN GENERAL SURGERY.

By P. Daneel, B.A., M.D., Cape Town.

More than half a century has elapsed since the introduction of chloroform and ether as general anæsthetics, and the disadvantages that arise from their use are too well known to need repetition. In spite of the skill that has been developed in the methods of application, deaths still daily occur, and this, combined with the other bye-effects of the narcosis, such as asphyxia, vomiting, nausea, dyspnœa from excess of mucus in the trachea and bronchial tubes, pneumonia and chloroform nephritis, has stimulated the desire to find other methods.

With much thought and labour other methods have been discovered, but the end in view has only been attained to a certain extent. The advance that has been made is, comparatively speaking, very small, but science seems to be here, too, on the triumphal road, and we expect further developments in the course of time. The following can be considered the chief advances that have been made in late years:—*

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* The intravenous hedonal and ether narcosis is of too late a date to allow of being added here.

1. The particular attention that has been paid to the manufacture of pure anæsthetics.

2. The systematic development of the methods of application and variations in the depth of the anæsthesia (*e.g.*, automatic drop-apparatus, Vernon-Harcourt, Roth-Dräger.)

3. The introduction of narcosis with combination of preparations (*e.g.*, ether or chloroform together with oxygen, morphia-scopolamine, or veronal).

4. Replacing the general anæsthesia as far as possible by (*a*) lumbar anæsthesia, and (*b*) local anæsthesia.

I have elected to deal with this last method, and in particular with local anæsthesia.

All the modern methods which have been adopted act either mechanically or chemically. Of the former great importance has been attached to the pressure on nerves, and several surgeons report amputations painlessly carried out by pressure on the peripheral nerve trunks. At first the pressure was applied by means of the hands or of specially-constructed apparatus with pads; later the Esmarch's bandage was used for this purpose. Another physical method was the ether spray, first introduced by Richardson in 1865. This has been replaced to a great extent by ethyl chloride. Other preparations have been introduced in late times, *e.g.*, methyl chloride, anæstol, anæstyl, metathyl and koryl, which are all more or less mixtures of ethyl chloride, besides carbonic acid gas in fluid or solid form. Speaking from my own experience, I prefer to incise without the spray, as the actual pain seems to be less than that occasioned by the thawing.

Local anæsthesia has passed through three successive stages since the discovery of cocaine, which very materially differ from one another. During the first period cocaine solutions were injected into the tissues, and one attained a satisfactory local anæsthesia, which would have developed more rapidly but for the toxic properties of cocaine. In the second period Schleich used very small doses and very diluted solutions of cocaine, but attributed to the solutions entirely hypothetical physical properties which replaced the specific effects of cocaine. He avoided the dangers of cocaine poisoning, but his solutions did not suffice to anæsthetise most of the operation areas. The third period is marked by the discovery of new preparations replacing cocaine and the introduction of suprarenin. The latter gives to comparatively dilute solutions of these substances properties which permit them to be injected in large quantities. The effects thus obtained far surpass those obtained in the first period when relatively concentrated solutions of cocaine were used.

According to H. Braun, local anæsthesia, excluding spinal anæsthesia, is generally classified under two headings,—terminal and conduction anæsthesia (nerve-blocking). In 1908 Bier introduced a method of intravenous anæsthesia. After rendering the extremity bloodless and applying an elastic bandage at two points, a subcutaneous vein is laid bare between these two points and the anæsthetic fluid (50 c.c. $\frac{1}{4}$ to $\frac{1}{2}$ per cent. novocaine solution) injected. This diffuses through the walls and causes, either "directly" or "indirectly," complete anæsthesia of the part between the bandages.

This method, although giving good results under favourable circumstances, is too complicated, and has not been adopted in hospital practice. The other original method of local anæsthesia, however, has been developed further, and in the course of time we have learnt certain essentials for the successful practice of it:—(1) It has been found that only solutions should be used which have as far as possible the same osmotic pressure and the same freezing-point as the tissue fluids. As the substances used for local anæsthesia are used in weaker solutions than the physiological concentration, the distension effects and the consequent damage to the tissues must be avoided by the addition of a sufficient quantity of an indifferent salt such as sodium chloride. (2) The retardation of absorption. This was first practised by Corning in 1889, later by Oberst, and consisted in tying off the particular part and rendering it bloodless by means of an elastic bandage. Braun at a later period added an extract of the suprarenal capsules (adrenalin, homorenin, etc.) or the synthetic compound suprarenin, to the anæsthetic solutions. The retardation of the absorption has the double advantage that less of the anæsthetic substance is required to procure a given effect, and that even large quantities may be used with impunity. (3) The study of the comparative sensitiveness of the tissues, first commenced by Lenander, has an important bearing on local anæsthesia. These researches are not yet concluded, but I may roughly mention the following groups:—

Very Sensitive.—Skin; periosteum; joint capsule; synovial membranes; ligaments; mucous membrane of rectum; urethra; parietal peritoneum; parietal pleura.

Moderately Sensitive.—Subcutaneous tissues; muscles; sheaths of tendons; aponeuroses; mucous membrane of mouth, nose, throat, antrum of Highmore, frontal sinus, ear.

Slightly Sensitive.—Bone; bone marrow; mucous membrane of bladder, uterus; dura mater.

Insensitive.—Tendon tissue; cartilage; mucous membrane of stomach, intestine, trachea; brain; visceral peritoneum; omentum; mesentery; gall bladder; kidneys; liver; visceral pleura; thyroid gland.

(4) A great advance was made in replacing cocaine by other preparations. The maximal dose, according to the Pharmacopœia Germanica, is 0.05 gram to $\frac{3}{4}$ gr. per dose. Several writers put it higher (up to 0.3 to 0.5 grs.), others less (0.01 to 1/6 gr.). The poisonous qualities of cocaine, the idiosyncrasy of many people against this preparation, and the experience that solutions stronger than 1 per cent. are liable to be dangerous, constitute momentous barriers against its general use. Those who have seen a death due to cocaine always retain a suspicion against this drug.

Now we come to the practical application of local anæsthesia, and in connection with this we may consider the properties of cocaine and the various preparations which have been suggested to replace it. Besides cocaine I wish specially to consider, for the sake of comparison, eucaine-B, tropacocain, alypin and novocaine.

Cocaine.—The evolution of cocaine as a local anæsthetic has been very gradual. At first very concentrated solutions (2 to 10 per cent.) were used for injection, with the result that many cases of poisoning were reported. The various phases of cocaine poisoning (after different methods of application) have been published from time to time, and it has been found that when the blood passing through the central nervous system contains the alkaloid in a sufficiently concentrated solution, if only for a moment, a general poisoning takes place. It is remarkable how dangerous this substance is when applied to cavities coated with mucous or serous membrane, *e.g.*, the urethra, bladder, scrotal cavity; in these instances the injection of a few c.c. of a 1 to 5 per cent. solution has frequently proved fatal.

The question whether cocaine poisoning can be prevented has practically been solved, and at present one can use dilute solutions in general surgery with impunity. In the course of research it was found that to obtain this object it was necessary to —(*a*) dilute the solutions sufficiently, and (*b*) retard the absorption by slowing down the blood-stream in the injected parts by means of Esmarch's elastic bandage, or by adding adrenalin to the injected solutions.

The maximum dose of 0.05 gram ($\frac{3}{4}$ grain) as laid down in the German Pharmacopœia ($0.03=\frac{1}{2}$ grain of the British Pharmacopœia) does not guard against poisoning, nor does it represent the largest quantity which can be injected without danger. Reclus has proved in over 7,000 cases that one may even double that quantity by using $\frac{1}{2}$ to 1 per cent. solutions. He injects as much as 0.2 gram (3 grains), but observes certain conditions under which this should be done,—(1) The cocaine should only be injected while the patient lies in a horizontal position. (2) The patient should remain on his back from two to three hours after major operations and for twenty minutes after minor operations. (3) While injecting, the needle should invariably be in motion in order to prevent the injection of large quantities of cocaine into the veins.

For cavities such as bladder, joints, etc., the question of dosage has also been solved, as it is found that these surfaces are equally well anæsthetised by contact with 0.1 to 0.2 per cent. cocaine solution for a sufficiently long period (say twenty to thirty minutes) as with a 10 per cent. solution. For the urethra 2 to 3 c.c. of a 2 per cent. solution of cocaine with adrenalin suffices.

In order to sterilize cocaine solutions they are best allowed to simmer up once, or the powders are dissolved in alcohol and the latter allowed to evaporate (Mickulicz). The solutions themselves are best prepared from powders which have previously been carefully weighed (say $0.05=\frac{3}{4}$ grain). This amount can then be dissolved in 10 or more c.c.ms. of normal saline according to the solution required, and 8 to 10 drops of suprarenal extract added. The suprarenal extracts or their equivalents used are either those of Parke, Davis, Burroughs Wellcome, or that of Meister, Lucius and Brüning (Hoechst). The cocaine tabloids of Pohl in Schönbaum (Danzig) or Burroughs Wellcome and Co. also answer their purpose. Parke, Davis has combined the cocaine with adrenalin under the name of codrenine. This latter must be diluted from 2 per cent. to $\frac{1}{2}$ per cent. when infiltrating large and deep areas. For mucous membranes the combination of cocaine (1 to 5 per cent. solutions) with ethyl chloride is a useful one.

EUCAINE-B.—Eucaine-B is either used in the form of the hydrochloride or in the form of the lactate, which is easily soluble in water. It has been found that in order to attain the same anæsthetic effect with eucaine-B as with cocaine, the eucaine-B solution has only to be slightly more concentrated than the cocaine solution. As the diffusive action of eucaine-B is less than that of cocaine, it means that the amount of toxic substance injected in a given case is not less with eucaine-B than with cocaine. A disadvantage which eucaine-B shares with tropacocaine is that it partly destroys the constrictory action of the suprarenal extracts on the vessels. A real advantage is that eucaine-B solutions can easily be sterilized by boiling and be kept unchanged for a considerable time. The action of eucaine-B as an anæsthetic for the bladder or urethra generally proves very inefficient. The eudrenine of Parke, Davis represents a 1 per cent. solution, and is put up ready for injection.

Tropacocaine.—The effect of tropacocaine as a local anæsthetic compared with that of cocaine passes off more quickly, and it is therefore necessary to use five to eight times stronger solutions in order to achieve the same anæsthetic effect. Although tropacocaine is less toxic than cocaine, this advantage, as in the case of eucaine-B, is outweighed by having to resort to comparatively more concentrated solutions. It has also been found to neutralise in a very high degree the action of adrenalin, and cannot therefore be used to advantage in conjunction with this substance. Usually 40 to 50 c.c. of a ½ per cent. solution can be injected.

Alypin.—As a local anæsthetic alypin has lately been used to a very large extent. It is only half as poisonous as cocaine, but does not possess the disadvantages of tropacocaine. It only very imperceptibly neutralises the effect of adrenalin, and as an anæsthetic acts more powerfully than tropacocaine. Dr. Paul Flessig (*Med. Klinik, No.* 5, *Jahrg.,* 1910) has recently revised seventy original articles on the use of alypin as a local anæsthetic in various branches of surgery. From his publication it appears that alypin has been employed in thousands of operations in 1 to per cent. solutions without any ill effects. Wilms in Basle reports two cases where a slight necrosis of the skin resulted after extraction of foreign bodies. Usually 0.05 to 0.2 gram in ½ per cent. solution combined with 5 to 10 drops adrenalin or suprarenin is used. I have used the preparation in several minor operations with entire satisfaction, and can recommend it as a local anæsthetic.

Alypin is placed on the market in crystal form in ½-gram flasks by Friedr. Bayer and Co. (Elberfeld). Lately tablets containing respectively 0.02 and 0.05 gram with suprarenin have been issued by Pohl (Schönbaum).

Novocaine.—Of all the substitutes for cocaine, novocaine has perhaps been used more extensively than any other of late years. Judging from hospital reports, it seems to have supplanted cocaine and the other anæsthetic preparations to a very great extent, and it is continually gaining ground. This is owing to the fact that it fulfils all the requirements of an ideal local anæsthetic. It is about seven times less toxic than cocaine, three times less than stovaine (Biberfeld), and about three and a half times less than alypin; it does not irritate the tissues; it can be easily sterilized without undergoing decomposition, and it combines readily with adrenalin or suprarenin. I have used it exclusively as a local anæsthetic in the last few years, and have no reason to complain of the results obtained. The dose, 0.5 gram (¾ grains), is usually regarded as the maximum when used hypodermically, but I have injected almost double that quantity with no evil effects.

Liebl administered to himself 1.15 grams in a 10 per cent. solution and noticed a slight discomfort, which gradually disappeared in the course of one and a half hours. During the amputation of the breast with removal of the glands in the axilla in a corpulent patient suffering from myocarditis, Hesse injected 250 c.c. of a ½ per cent. solution, *i.e.*, 1.25 grams. At the end of the operation, which lasted about one and a half hours, a slight collapse set in which disappeared on administration of the usual stimulants (camphor, alcohol) in an hour.

Novocaine appears on the market in the form of crystals, as tabloids with suprarenin, or in solutions with suprarenin (Meister, Lucius and Brüning, Hoechst). For our purpose the tablets marked A and B are best suited. Each tablet (A) contains 0.125 gram novocaine, 0.00016 gr. suprarenin borate, and gives, dissolved in 50 c.c. saline solution, a ¼ per cent., in 25 c.c. a ½ per cent. solution. Each of the tablets (B) contains 0.1 gr. novocaine and 0.00045 gr. suprarenin borate, and dissolved in 5 c.c. saline solution gives a 2 per cent., in 10 c.c. a 1 per cent. solution.

For infiltration the ¼ to ½ per cent. solutions are best suited, while in circular anæsthesia the ½ to 1 per cent. solutions, and in peri- and endo-neural anæsthesia the 1 to 2 per cent. solutions are used. The solutions are best prepared immediately before the operation. The tablets are, according to the manufacturers, sterile, but it is safer to allow the solutions to boil up once or twice. Boiling the solutions for any length of time destroys the active principle of the suprarenin. Lately, Löwen (*Münch. Med. Woch.*, No. 39) has, on the recommendation of Gros, introduced the bicarbonate of novocaine as a local anæsthetic into surgery. It promises to be very useful in the blocking of large nerves, as, according to Löwen's experience, it has an anæsthetic action two to three times stronger than the chloride, which at present is used very extensively. Syringes that have been boiled in a soda solution must be washed in sterile water or saline, as the soda precipitates the novocaine. About ten minutes after the injection the anæsthesia is complete and the operation can be commenced. The anæsthesia lasts with ½ per cent. solution from one and a half hours to two hours, and after two to five hours pain from the operative interference begins to be felt, and may be very severe under certain circumstances.

The Practical Application of Local Anæsthesia.—The incision of an abscess, or even the removal of a wart, can be done without any anæsthetic. In some cases the ethyl chloride spray will do good service. If, however, continuous cutting or dissection be necessary, as in the extirpation of a tumour, the extraction of an ingrowing toe-nail or the removal of foreign bodies, one must resort to the injection of anæsthetic fluids. In these cases two methods are at our disposal; Firstly, the infiltration method by means of which the ends of the sensory nerves are rendered insensitive through infiltration with the anæsthetic compound (Schleich, Reclus); secondly, the conduction method by means of which the nerve trunks are blocked. Here we distinguish between an endo- or peri-neural conductive anæsthesia according as the anæsthetic is injected into the nerve itself or in its immediate vicinity.

The original method of Schleich, which consisted in the injection of very large quantities of diluted cocaine solutions into the skin itself and afterwards successively into the tissues in the course of the operation, has been fairly abandoned, as experience has shown that the anæsthesia was as a rule insufficient. In the conduction methods it is best to make the injections directly into the nerves themselves, but only a few nerves are so situated that this is possible, and to expose the nerve directly with an incision, as was done in the beginning, is not carried out any more.

The kataphoretic action of electricity has been tried for the production of local anæsthesia, and minor operations have actually been performed with it, but the method has not been able to survive in spite of being strongly recommended by several observers.

In these cases the peri-neural injections which have been specially used by Hackenbruch for his "circular analgesia" serve as a substitute, and act best where the branching of the sensory nerves takes place. For the large nerve trunks of the extremities, all observers agree that these peri-neural injections do not serve their purpose, as the time taken is too long (up to an hour) until the conductivity is completely blocked. The constriction of the extremities in these cases cannot be dispensed with, and the lengthy constriction of the leg or arm, even with very careful manipulation, is for the patient a very troublesome and painful process. It is only in exceptionally thin limbs that the necessary pressure of the elastic bandage is slight enough to be tolerated for any length of time without discomfort.

For the successful practice of local anæsthesia it is necessary to be careful regarding certain important points. Nothing will do so much harm as its application without proper selection of cases and an incomplete knowledge of its technique. Not only is the injection technique to be mastered, but the operative technique has to be developed on special lines. The operative technique requires of the surgeon more time, patience and self-restraint. A very exact hæmostasis must be secured and a careful manipulation of the tissues is necessary. Dragging and fraying of the tissues and rough manipulations in general have to be avoided. For impatient "record-breaking" operators the local anæsthetic has not been discovered.

The injection technique endeavours to distribute the anæsthetic solutions primarily in such a manner as to cause a complete demarcation of the operative field from the sensitive areas. By doing this one is not bound down to the line of incision, and may freely modify the technique during the operation. As a rule one marks out the field of operation by depositing small quantities of the anæsthetic in the cutis vera at certain intervals by means of a syringe with a thin needle. From these points the deeper infiltration is carried out by means of a thicker needle. After inserting the needle a few drops are injected hypodermically, the needle is then pushed onward and another injection made, and so forth until the infiltration is complete. This has the double advantage that the injection of a large quantity of the anæsthetic solution in an accidentally punctured vein is prevented and that the injections themselves are painless. Fasciæ and tendons do not require a separate infiltration, as subfacial or paratendinous deposits of the fluid suffice to render them anæsthetic.

Extensive bone operations are only possible when the whole part of the body in which the area of operation lies is rendered anæsthetic by blocking the innervating nerve trunks. The same applies to joints.

The syringe used for the injections requires special attention. It should always act well, and the fittings should be secure in order to permit injections into dense or hard tissues needing pressure. The needles must be thin and firm, the pointed ends not too long but short and sharpened obliquely. Two needles, the one straight and the other bent at an angle, suffice. The platinum-iridium needles should not be too soft and flexible. After use the needles should be washed with absolute alcohol and then dried by blowing air through them. They are best kept exposed to the air with a wire drawn through them. Metal syringes are inconvenient, as one has no indication regarding the contents. Luer's syringes break easily and are expensive. The "Record" syringes break easily in sterilising and can only be repaired with difficulty. I have always found the ordinary glass syringes, of 5 to 10 c.c. capacity and working with an asbestos piston, the best.

Patients to be operated on under local anæsthesia do not require any special preparation previously. Should there be any uncertainty as to the success of the local anæsthesia, they must, of course, be prepared as usual for a general narcosis. In these cases it is perhaps best to renounce the idea of the local anæsthetic altogether. In extensive cases a little morphia or veronal will greatly assist in quieting the patients.

INDICATIONS FOR LOCAL ANÆSTHESIA.—Local anæsthesia comes as a special boon to the general practitioner, as all minor aseptic operations can be performed with it. It is most acceptable in those cases where there is a large deficiency of hæmoglobin, or where diseases of the heart, lungs, arteries, or metabolism make a general anæsthetic undesirable. Children are, as a rule, to be excluded. It is essential to exclude from local anæsthesia all diffuse, infiltrating and progressive processes, such as malignant tumours, phlegmonous deposits and deep-seated progressive whitlows.

Not merely has minor surgery become the domain of local anæsthesia, but even large operations on the intestinal tract are successfully performed under it. These are, however, exceptions, and do not belong to its real sphere. Each surgeon will with increasing practice increase the extent of its application, but indiscriminate practice of the method is much to be deprecated.

I. *Operations on the Head.*—One is often able by means of a circular linear injection at the level of the eyes to attain a complete anæsthesia which reaches far beyond the hair border, but sometimes one is unable to use this method for operations, on account of the great variation in the nerve supply. Generally one is compelled to inject round about the field of operation. The majority of the operations on the head have been carried out under local anæsthesia, even the double resection of the upper jaw (Matas). I have used a local anæsthesia, with benign tumours of the scalp, in the treatment of scalp wounds, the extirpation of cancroids (rodent ulcers), and in plastic operations. Braun also recommends local anæsthesia for extensive trephinings in brain tumours, etc., and recommends especially the favourable action of suprarenin on the vessels of the head. Kilian's opening up of the frontal sinus is often carried out under local anæsthesia. In this it is necessary to anæsthetise the nasal nerves (branches of the first division of the fifth nerve) in the orbit. For this purpose a long needle is introduced above the inner canthus of the eye, and keeping close to the bone, about 5 to 10 c.c. of the ½ per cent. solution is deposited near the inner wall of the orbit. Some of the same solution is also injected immediately on to the periosteum along the upper border of the orbit and the whole frontal surface of the frontal sinus, then the operation can be commenced in ten minutes' time.

II. *Mouth.*—Local anæsthesia in tooth extraction has been very extensively practised. For solitary extraction subperiosteal injections are made, but, as these sometimes cause much pain, where a whole row of teeth requires extraction the lingual nerve and inferior dental nerve before entrance into the bone canal are anæsthetised.

In order to anæsthetise these two nerves it is best to locate first the prominent tubercle (lingula mandibulæ) about 1½ c m. above the third molar tooth. At this point the needle is inserted and pushed onwards close to the bone and parallel to the row of teeth of the lower jaw, and injections are made at certain intervals. First the lingual nerve is struck, which lies between the front edge of the bone and the pterygoid muscle. About 1½ cm. further on the inferior dental nerve is met with before its entrance into the bone canal. For the whole jaw both sides have to be anæsthetised, combined with injections round about the bone. This method allows partial resections of the jaw. Other operations in these parts which are easily amenable to local anæsthesia are the opening up of empyema of the antrum of Highmore from the canine fossa, the chiselling off of epulides, the extirpation of tumours and tuberculosis of the tongue and of cancers and cysts.

III. *Operations on the Throat.*—As it is easily possible to render the hole of the front of the throat anæsthetic, neither the extent of the field of operation nor the length and difficulty of the same forms a contra-indication against the use of local anæsthesia. But in my opinion, in the case of large adhesive glands we shall always be forced to make use of chloroform or ether, as the operation has usually to be discontinued in the deeper parts in order to allow a renewed infiltration to be made. On the contrary, we consider laryngectomy, laryngotomy, the extirpation of small collections of glands, small tumours, cysts, the majority of benign goitres as operable under local anæsthesia. In laryngotomy and laryngectomy it is important to strike the vicinity of the superior laryngeal nerve at the lateral edge of the hyoid bone, as the sensibility and the reflexes of the entrance and inside of the laryngeal box and of the mucous membrane of the pharynx which borders on this are abolished. After this injections are made round about the laryngeal box, and the operation can be performed without pain. For laryngectomy about 100 c c. of the ½ per cent. solution is used. In œsophagotomy local anæsthesia is also useful on account of the prevention of possible vomiting.

Until quite recently the use of local anæsthesia in goitre extended only to the infiltration of the subcutaneous tissues, and the operator was bound to depend on the good will and determination of the patient which enabled the latter to stand the pain caused by the dislocation of the tumour from its bed. Now. however, with improved technique it is possible to operate upon almost every goitre painlessly under local anæsthesia. Occasionally one hears complaints of dyspnœa during certain stages of the operation.

IV. *Operations on the Breast.*—Nowadays one would almost consider it a mistake to undertake the resection of a rib with pleural empyema, gangrene or abscess of the lungs under general narcosis. The heart is not capable of resisting the effects of the chloroform and at the same time of adapting itself to the changes of pressure, and therefore local anæsthesia comes as a boon, especially if a morphia injection be administered beforehand. Chronic cases of tuberculosis of the ribs where it is not possible to define the limits of the field of operation, and where all the tissues are riddled with fistulæ, are not suited for local anæsthesia. All benign non-purulent affections of the breast, solitary or multiple fibro-adenomata, cysts, well-defined indurations in interstitial mastitis, also come within the scope of local anæsthesia. On the other hand, the malign breast tumours are excepted from local anæsthesia, as the thoroughness of the operation in clearing out the axillary glands might be risked.

V. *Abdominal Operations.*—Several typical abdominal operations have already been done under local anæsthesia, but the performance of these depends on a number of circumstances which have to be considered in each individual case. The same rules apply to the operations on the abdominal wall as on other parts of the body, but when the abdomen is opened other factors come into play which have to be considered. The psychical status of the patient has to be regarded carefully. From the experiments of Lennander we know that the visceral peritoneum and the organs covered by it are not sensitive to pressure, pinching, cutting, etc.; only exceptionally does the mesentery show a sensitiveness to pain. With very corpulent patients it is practically impossible to work with any degree of certainty at a depth. For these stated reasons it is evident that where a tugging at the stomach on account of adhesions is necessary, where the intestines are adherent to the parietal peritoneum, where the patient is very nervous, where it is necessary in large operations to work at any depth, it will be necessary to employ a general anæsthetic. Local anæsthesia, on the other hand, can be employed when it is possible to carry out the operation with gentle manipulation. This is generally the case in anterior gastroenterostomy, in gastrostomy, in making an artificial anus, in bringing tumours of the sigmoid flexure before the abdominal wall, abscesses of the liver in echinococcal cysts, and in Talma's operation. Although I have often seen gastro-enterostomy and even resection of the stomach done under local anæsthesia, these operations ought, in my opinion, to be carried out under a general anæsthetic. I agree, however, that it is often possible to perform these last-named operations on patients who otherwise cannot stand a general anæsthetic. In appendicitis it will be wise to limit local anæsthetics to the opening of abscesses which are near the abdominal wall. In ordinary cases of appendicitis I invariably use a general anæsthetic, as with local anæsthesia the appendix cannot be brought to the surface in every case, and the gridiron incision is not well adapted for it on account of the forcible stretching of the different muscle layers.

Formerly one refused to operate on elderly patients with large umbilical or inguinal herniæ, and they were sent away with bandages. That now has changed since local anæsthesia renders excellent service in these large adherent herniæ where, in order to avoid a possible hypostatic pneumonia, one is afraid to risk a general narcosis. I have operated for herniæ the size of a man's head in old men and women past seventy, and have allowed them to sit up in a chair on the second day. The size of the hernia is no contra-indication, and I operate for hernia almost exclusively under local anæsthesia when the psychical status of the patient allows it. To operate on hernia without pain it is necessary to study the correct injection technique and to proceed systematically.

VI. *Gynæcology.*—In gynæcology local anæsthesia is very seldom used, but it is sometimes very useful, as in the extirpation of vaginal and urethral cysts, for colporraphy, stitching perineal tears, and for small plastic operations.

VII. *The Male Genital Organs and Urinary Organs.*—The majority of operations in this region can be done with local anæsthesia, *e.g.*, phimosis, hydro-, hæmato- and varicocele and castration. It will always be of advantage to anæsthetise the spermatic cord. In the case of abscesses or urine infiltration it is best to use a general anæsthetic. In castration and in operations for hydro-, varico- and hæmatocele it is important to anæsthetise the whole half of the scrotum with cord and septum. In phimosis operations it is important not merely to inject into the skin round about the sulcus coronaria, but also the foreskin. This is brought about by inserting the needle on a grooved director between the glans penis and the prepuce and puncturing at the reflection of the preputial skin. Of the operations on the urinary organs it is specially necessary to mention the suprapubic cystotomy and external urethrotomy. Lant of Amsterdam even advocates local anæsthesia in prostatectomy.

VIII. *Operations on the Rectum.*—With very nervous patients the practice of local anæsthesia in these parts is often associated with many difficulties. As the regions are very sensitive, even the introduction of a very thin needle causes extreme pain, and local anæsthesia is therefore unsuitable. With intelligent patients local anæsthesia easily suceeds and is well adapted for the operation of hæmorrhoids, the ligature of rectal polypi. The anæsthesia is best obtained by a circular injection round the anus, then the deeper layers are infiltrated while the point of the needle is advanced towards the rectal mucous membrane and the second finger of the left hand inserted into the anus, directing the needle point so as to prevent puncture of the mucous membrane itself. When the mucous membrane is very sensitive, as in the case of fissures, it can be rendered anæsthetic by inserting cotton-wool tampons previously soaked in a cocaine-adrenalin solution. On account of the depth to which general anæsthesia has to be pushed in operations on the perineum and anus, local anæsthesia, where it can be employed is of distinct advantage. No collapse need be feared, and an additional advantage is the small amount of bleeding.

With anal fistulæ I would not advise a local anæsthetic, as I do not think it practicable to introduce needles alternately into infected inflammatory and healthy tissue. Several cases have been recorded where, in simple external rectal fistulæ, large and obstinate abscesses between the rectum and the sacrum have been brought about.

IX. *Extremities.*—In the polyclinics, where local anæsthesia can be used to a very large extent in the surgery of the extremities, it has taken firm root of late years. In the neighbourhood of mines, factories, etc., where injuries to the toes and fingers happen so frequently, it is much appreciated by medical men. The majority of disarticulations of the toes or fingers, the extraction of foreign bodies, the opening of whitlows of the terminal phalanges, are done, operations on the fingers and toes are carried out, after injections at the bases of the digits. When parts of the middle hand or middle foot fall in the field of operation circular injections are made higher up on the forearm, and at the time peri- or endo-neural injections into the nerves themselves. On account of the suprarenin contained in the novocaine tablets, it is unnecessary to render the digits bloodless. Local anæsthesia may also be used in the extirpation of bursæ, small tumours (as a rule benign), the treatment of wounds of the soft parts, and the suture of tendons.

Amongst in-patients it serves in cases of suture of the patella and arthrotomy of the knee-joint for floating cartilages. Even amputations of the leg or arm I have performed without pain under local anæsthesia, but I should not advise this procedure as a general rule, as the injection technique is difficult and the anæsthesia is often complete only after a very long time. Besides, the psychical shock is very great. On the other hand, the puncture of joints for drawing off or injecting fluids, the removal of menisces from the knee-joint, I almost exclusively do with local anæsthesia.

The number of operations carried out in hospital practice by this method is steadily increasing from year to year. At present about a fifth to a quarter of all operations in well-appointed institutions are carried out under local anæsthesia. With increasing practice each individual surgeon will soon learn to cope with the inconveniences of local anæsthesia, and will gradually widen its scope. The inconveniences do not weigh heavily in the balance against the advantages.

For the general practitioner the method seems to have a special advantage in that it dispenses with the services of an anæsthetist. He is able to leave the patient immediately after the operation and need not wait till consciousness has returned, and since there is no vomiting there is no danger of aspirating any stomach contents. It is best for the operator not to speak during the operation, as patients listen very attentively to every word and as a rule misconstrue what is said. On the other hand a few sympathetic remarks never fail to have a soothing effect.

IX.—THE TREATMENT OF FRACTURES BY MASSAGE AND MOVEMENT.

By J. Bruce-Bays, M.D. (Lond.), Grahamstown.

(*Abstract.*)

Until comparatively lately the treatment of fractures has remained the same for many hundreds of years—splints and immobility. Often this led to more serious results than did the actual fracture, owing to wasting of muscles and contraction of ligaments and tendons rendering the limb useless for some time after the fractured bone had united. This is obviated by massage of the limb, commenced within a few hours of the injury, and by passive

movement in a few days with active movement soon after. The blood is thus kept in motion, all effusions are absorbed, the muscles retain their full vitality, and no contraction of ligamentous structures takes place. As accurate an apposition of the bones as possible is obtained by means of light splinting, which is left off as soon as union of the bone is obtained. The results of this treatment as far as regards the utility of the limbs is far in advance of those of the older methods. Massage, strapping and immediate movement are useful also in cases of sprains. Advisability of not allowing patients with fracture of neck of femur to remain in bed, and also the very satisfactory results of the treatment of Colles's fractures by massage. Pioneers of the treatment were Wharton Hood in England and Professor Lucas-Championnière in France. The safety of massage as opposed to immediate suture was pointed out, as although this secured a more perfect apposition of the bony ends, there was an element of serious risk, which in most cases forbade the operative treatment.

X.—THE OPERATIVE TREATMENT OF SIMPLE FRACTURES.

By H. R. Mullins, M.R.C.S., L.R.C.P. (Lond.), Grahamstown.

This subject may conveniently be considered under three headings: *First,* the cases in which operative measures ought to be resorted to. *Second,* the operative technique, including the preparation of the patient for operation, the necessary instruments, and the after-treatment. *Third,* the advantages of operative treatment over non-operative.

First, then, the cases in which operative measures ought to be resorted to. It has been Mr. Arbuthnot Lane's practice for many years to operate on any simple fracture in which he could not obtain accurate apposition of the fragments by the use of splints. The argument is often raised by opponents of the operative treatment that cases are operated on in which an equally satisfactory result might have been obtained by the use of splints. Mr. Lane's routine treatment is, *first,* to endeavour to obtain accurate apposition of the fragments by the use of splints. *Second,* to have X-ray photographs taken, and *then* to advise treatment, operative or non-operative, according to the results shewn by the pictures. This raises the point of X-ray photographs.

Before the days of X rays the profession was content to *set* bones, with varying degrees of skill, and to rest assured that the after-result would never be questioned by the patient, provided the fragments united and firm union was obtained in some position which was not flagrantly malicious. Now, however, X-ray work has reached a very high degree of proficiency, and a patient whose fracture has been treated by splints is quite likely to see X-ray photographs of the result at some future date. There have already been more than a few law cases, in which the practitioner has been sued for malpractice, and these cases are not likely to diminish in number, as the general public learns more clearly the meaning of fractures, and the ways in which they *can* unite.

But, although X-ray photographs may be distressing in telling the truth about some results of splint-treated fractures, they certainly have proved a very great boon to those advocating operative treatment. Speaking generally, a single X-ray photograph of any particular fracture may be entirely useless, or at least very misleading. To obtain an accurate idea of the displacement of the fragments, at least two photographs should be taken in planes crossing each other at right angles. These two pictures will give more information as to the actual position of the fragments than any surgeon can guarantee by his senses of sight and touch. A single photograph may shew the fragments in almost perfect apposition, and it may not be until another picture is taken at right angles that the real displacement is detected.

It has been argued that X-ray photographs exaggerate the displacement of fragments, but the knowledge gained by operative interference has certainly not borne this out. On the contrary, the tendency is to underestimate the difficulties of replacement of the fragments to their normal axis, because the X rays give no information of the soft parts which often intervene between the fragments and oppose reduction.

In advising operation, there are many other factors to be considered. There are, of course, some fractures which operation would not benefit, *e.g.*, intra-capsular fracture of the neck of the femur. The patient's constitution and age must be taken into consideration, and the strictest asepsis must be possible.

This leads to the *Second* heading, the operative technique, including the preparation of the patient for operation, the necessary instruments, and the after-treatment.

Before operating on any simple fracture, it is very important to most thoroughly cleanse the skin over and around the site of the fracture, allowing a very wide margin, *e.g.*, in operating on a fractured tibia, the lowered liimb should be prepared from the toes to the trochanter. Children's skins are easily prepared, but it is often necessary to delay operation in adults for several days before the skin can be called surgically clean. The surgeon and all those assisting him wear sterilised overalls, masks, caps, and rubber gloves which have been boiled.

An incision of ample length is made, and immediately after the skin has been cut, it should be carefully excluded from the wound by means of sterilised towels or pads, which are clipped to the margin of the incision by Lane's forceps. All instruments used should have *long* handles, so that there is no risk of even the gloved-hand coming into contact with the wound. Should the business-end of any instrument touch even the gloved hand, it must be boiled again. In short, the thoroughly cleansed gloved hand is treated as though it were absolutely dirty, and the operation should be performed without the hand being nearer than three inches to the wound. After manipulating the fracture into position, for which proper instruments are essential, the fragments are fixed by means of a steel plate or plates and screws which are threaded throughout their entire length. No particular care need be taken of the periosteum. Sometimes two or three sutures are required to draw the deep fascia together, but no ligatures are necessary. The wound is closed by means of Michel's clips. Dry sterile pads are then applied as dressings, the limb is placed in splints, and the patient returned to bed.

About forty-eight hours after operation, there is often a slight rise of temperature, due most probably to reaction, but this comes down to normal after two or three days. The clips are removed on any day between the sixth and the tenth after operation, when the wound is dressed for the first time. As a rule the limb should not be used for about six weeks. *Lastly,* what are the advantages of operative over non-operative treatment? The main advantage gained by operative interference is that the fragments are restored to their original position, and the bone is then capable of performing its normal functions, and working through its original axis. There is no doubt that if the operations were easy, and there was no danger of sepsis, they would have long ago become popular. Thus we have to stake the chance of sepsis against the likelihood of a perfect limb. With our present knowledge of surgery the chance of sepsis is infinitesimal, but it must be again impressed on operators that asepsis with bones is much more necessary than asepsis with abdominal or like operations.

Discussion.

Dr. Ritchie Thompson said Lane's views were becoming more widely accepted. Ordinary methods give nothing like the same adjustment. The whole point is the question of asepsis. The method should not be adopted by all and sundry, even though the X rays showed much displacement. We were all trained in the school that the first essential of the treatment of fractures is rigidity. Now there are two methods—either treatment by massage or Lane's methods, which secure absolute immobility.

Dr. B. Hewat enquired what the term of quiescence by Lane's method was, as he considered it reflected on the operation if this were not reduced. A steel plate used too soon is likely to work loose. If due apposition of the fragments can be obtained without surgical interference, he considered it infinitely better to continue treatment by the old methods.

Dr. Parker advocated Lane's method in cases where tissues were intervening between the bony fragments, as, for instance, in fractures of the shaft of the femur due to indirect violence where some fibres of the quadriceps intervened. He remarked that you get a scandal after every fracture you set in these days of X-rays, which, however, soon dies down.

Dr. Keegan said there were three methods adopted in the treatment of fractures:—(1) splints; (2) massage (the French method); (3) cutting down and wiring. He believed that Lane's method had come to stay, and to be practised by the general practitioner. He did not believe that every simple fracture should be cut down upon and wired. In cases where there is delayed union he did not see how to get over the operative method. In cases of ordinary fracture, the results obtained by the old method of treatment have been sufficiently good to justify their continuance. He did not think one needs to be so absolutely aseptic as reader of paper suggests. Strongly objected to absolute insistence by everybody of necessity of using gloves. In his own cases, with wiring, he has achieved excellent results, beyond the fact of a certain amount of shortening. In all cases where there is delayed union he considered it the duty of every man to wire.

Dr. Gow said Lane's results were due to the admirable technique and asepsis which he insists on. He wondered whether it is absolutely necessary to obtain so complete apposition of the fragments as Lane demands. The function of the limb is often not interfered with, especially if adjusted under general anæsthesia. The only fracture where he would adopt operative treatment from the beginning would be fracture of the femur. He considered the early use of massage and movement of extreme importance. One week after the fracture he applies massage with satisfactory results.

Dr. Davies (Pretoria) said that since the introduction of the Workman's Compensation Act the public have become more critical of the results of the treatment of fractures. He has adopted Lane's method in two cases with good results. One of the cases was a simple fracture of the tibia and fibula in a medico-legal case. There was muscle and blood-clot between the fragments, and it was impossible to secure accurate apposition. He used the old method of wiring, and obtained a good result functionally and anatomically. In ordinary fractures of long bones where the case is likely to come into court he believed it to be the best course to adopt.

Dr. Daneel agreed with Dr. Ritchie Thompson in confining the primary suture of fractures after Lane to well-directed hospitals where there is a single control of the nursing staff. For certain fractures the treatment according to Barden-Lever or Zuppinger gave very good results.

Dr. Mullins, in reply, said that fractures about joints should be always treated by massage and movement. After Lane's operation a small percentage of patients return to hospital complaining of the plates giving pain, and have them removed. These are plates which are immediately subcutaneous, as on the inner surface of the tibia. The operation of removal is simple, and the patient leaves hospital in four days. With Lane's operation it is necessary to have some trained assistance, although the operation is not as a rule difficult. Cases remain quiescent for six weeks after the operation, varying according to the fracture. If there is good apposition, do not interfere, but treat by splints. Cases of non-union are more difficult to treat than recent cases, as they require some freshening up at the edge, etc. Lane never uses his method in cases of Colles's fracture. The plates are left in in ninety-eight per cent. of cases, and are only removed where rubbing takes place against the boot, the olecranon, etc.

XI.—APPENDICITIS.

By W. Russell, M.D., M.S. (Aber.), Kimberley.

I should like to mention that, judging from the case records of this hospital, appendicitis is becoming more frequent. I do not think this is due to a more accurate diagnosis, but that other causes contribute; I would suggest that the greater prevalence in recent years of eating cold storage meats and canned foods and fruits may have a contributory influence.

Pain.—At the commencement of an attack of appendicitis, the patient seldom locates pain in the right iliac fossa; as a rule it is felt all over the abdomen of a colicky nature, often referred to the umbilical region. Then perhaps in a few hours acute pain is felt over the region of the appendix; not infrequently the pain is referred along the anterior crural nerve, or in the spermatic cord or testicle.

The sudden outbreak of acute pain means that the parietal peritoneum is involved. If the severity of the pain continues grave changes may be taking place and urgent operation is required.

I saw an interesting case of this: Mrs. X. complained of colicky pains in the abdomen late one evening; next morning they still persisted; temperature and pulse were only slightly raised. About noon the pain was very acute, the patient screaming out in agony; she would not tolerate the slightest pressure over the right iliac region. The facial appearance suggested that the patient was very ill and in great distress. She was brought to hospital and operated on within five hours of the acute pain. The appendix was readily found covered over with plastic lymph and very much swollen. After the operation it was cut into with a knife, and out shot, under great pressure, about half an ounce of pus. The acute pain in this case was evidently due to the very swollen appendix with the pus inside under great pressure.

One requires to be very much on one's guard against a sudden cessation of pain; one may be readily deceived by that period of calm which not unusually follows, and be under the impression that matters are going on all right, and that the patient is recovering, and yet the condition inside may be that of a stinking and gangrenous appendix. You naturally ask yourself how you are to know whether an operation should be performed or not if this quiescent period occurs. In the first place, I should judge a good deal by the facial appearance; the pale face, the hollow orbits, the toxic look, will all help. Then the pulse may or may not assist you—it generally is accelerated; but the character, I think, is of more importance; the missing of a beat now and then is suggestive of toxæmia. If the temperature be high that will help you, but then, the temperature may be normal or even subnormal.

Let me give you an instance of a case I had in my early days when I thought more of pulse and temperature than I do now. An adult native was brought to hospital complaining of acute pain in the right iliac fossa; a lump could be felt there tender on pressure; there was no abdominal distension. Temperature and pulse were both about normal, but the toxic facial expression was even more marked than before. I operated that evening and removed a stinking gangrenous appendix; the patient died. I have always blamed myself over that case for not having operated sooner. I trusted too much to the pulse and the temperature.

Then there is the tenderness on pressure over the abdomen which one must elicit in a careful and methodical manner. Then you examine the condition of the pelvic peritoneum; your patient may say "I have generally pain in making water"; you make a rectal examination, and you feel a tender spot. I never omit to make a rectal examination in a case of appendicitis, and I have gained a good deal of knowledge by doing so.

A patient was admitted to hospital complaining of pain whenever he emptied his bladder, or when the bladder was full; he had previously been treated with bladder sedatives, but the condition persisted. He gave a history of an attack of appendicitis sometime before the bladder symptoms were apparent. An examination *per rectum* was made and a tender place was found by the finger on the anterior surface of the rectum. It was suggested to the patient that probably the tip of the appendix had become adherent to the bladder, and that an operation was the only thing to give relief: he consented, and the tip of the appendix was found adherent to the bladder as described. He made an uneventful recovery and complained no more of bladder symptoms.

In order to form a correct opinion as to the value of pain in the right iliac fossa, we must begin by ascertaining whether the pain is superficial or deep. The superficial pain is manifest on the slightest pressure, even pinching up the skin; this is mostly due to referred pain propagated from some point more or less distant from the spot under examination. A young coloured man was admitted to hospital looking very ill, with quickened breathing, high temperature and pulse, no expectoration or cough. Nothing could be detected of apparent importance on examination of his chest. His one complaint was acute pain in the right iliac region, not increased on deep pressure. The pain was so acute that opiates had to be given; this relieved the pain for a time, but the effect soon passed off and he was as bad as ever. He looked decidedly ill, and I thought it must be a case of appendicitis so I agreed to operate. To my surprise I found a perfectly normal-looking appendix, and with no inflammatory signs to be seen about the cæcum. I stitched up and got the patient back to bed. Next day the pain was much easier, and the patient commenced to cough; a distinct pneumonic patch was then found in the right lung, and a typical pneumonic crisis occurred a few days later.

Another interesting case I saw was a young neurotic Jew who complained of pain over the right iliac region; no tumour was to be felt; temperature and pulse both normal. He had had off and on for the previous eighteen months similar attacks of pain, and his condition was diagnosed by a couple of medical men as appendicitis, so he was sent to hospital for operation. I was not at all convinced that the appendix was at fault as he complained of severe pain shooting along the nerves of the right arm and hand, which I took to be neuritis; however, he was determined to have his appendix removed, and I operated to find that organ and the surrounding parts quite normal-looking. After the operation he continued to complain of the pains in the arm and hand; that in the iliac region had gone. Two months later he again appeared in the hospital with the old pain over the region of the appendix. Under medicinal treatment all pain left. I believe the abdominal pain was also of the nature of a neuritis. So evidently we have several maladies of the nature of appendicalgia, having the seat of pain over McBurney's point. Recent researches have shown that the origin of the appendix is generally some distance away from the painful spot, so naturally we ask ourselves what share must be accorded to appendicular pain in respect of the situation of the diseased organ.

Where is the appendix to be found—on which side of the cæcum? Quoting Lejars:—" If the pain on pressure is situated more towards the middle line or a little to the left, if there be an area of pain on the segment of the rectus muscle, the appendix probably runs downwards, lying somewhere near the promontory or behind the bladder, and the vesical pain, tenesmus, pain radiating down the urethra and felt on defæcation, will confirm this supposition. If movements of the thigh, especially flexion, are painful, we may suspect that the appendix has contracted adhesions with the psoas-iliac muscle. If, on the other hand, the appendix runs upwards behind the cæcum in the vicinity of the diaphragm, the diagnosis will be cleared up by finding respiratory disturbances and a sub-costal painful spot. When the appendix is lying free in the main peritoneal cavity, the pain is generalised and diffuse, and the vomiting supervenes early and is persistent.

Krüger says: "Early vomiting indicates that the appendix is in contact with the peritoneum of the main cavity, for it ceases as soon as the focus of suppuration is shut off and becomes localised."

In the case of an ascending retro-cæcal abscess the pain may centre in the right hypochondrium, or in the posterior lumbar region. A point beneath the right linea semilunaris about on a line with the anterior superior spine of the ilium is oftenest indicated as the most painful spot, and is the most tender on pressure; this is known as Clado's point, and is probably more accurate than McBurney's in showing where the base of the appendix is localised. But it is unsafe to assume that the appendix will be found beneath the spot which is said to be painful on pressure. Lockwood says when the inflamed appendix is in the pelvis he has usually observed that pain was referred to the umbilical region, and also elicited the greatest tenderness on pressure *per rectum.* One has to remember that in estimating the degree of tenderness and pain on pressure, the thickness of the abdominal wall has to be taken into consideration. I have known the pain to be referred to the gall bladder region and that organ examined for gall-stones, and pain about the hip-joint may simulate that disease, also a psoas abscess. We must also be on our guard about the muscular rigidity, which is greatest when both visceral and parietal peritoneum are involved. Lockwood narrates the case of a woman who had a gangrenous appendix, with diffuse septic peritonitis, but whose abdomen was soft and moved freely; apparently she suffered but little pain even when pressure was applied to the abdomen. Not infrequently the outer edge of the contracted rectus abdominis feels peculiarly round, and rolls under the finger like a swollen appendix: this should be obviated by following the golden rule of examining both sides.

Pulse, Temperature and Respiration.—Those should be considered together. The pulse is perhaps the most reliable indication of all. An increasing pulse-rate is an absolute command to operate; a rapid pulse accompanied by a low temperature is often of grave import. When the pulse-rate suddenly quickens to 110 to 112 and does not fall, the appendicitis has probably resulted in perforation, gangrene, or acute abscess. Yet one has met with cases in which the pulse-rate is from the beginning and throughout slow; those are generally extremely acute cases of septic poisoning as, for instance, the case of the native already narrated. A falling pulse-rate betokens a subsidence of inflammation. In adults with acute appendicitis a pulse-rate which exceeds 100 is a matter of some concern. In children a quick pulse may not have the same significance as in adults, but with them the rate rapidly alters, so that a very few hours' delay will tell whether it is increasing or diminishing.

The temperature taken by itself is seldom conclusive. A rigor of course nearly always means suppuration, and a temperature that keeps high for several days is good evidence that the case is a serious one. Yet even when there is a large abscess the temperature may scarcely rise above the normal owing probably to the fact that there is in those cases so little tension.

Two conditions we have, however, to watch:—(1) A sudden rise of temperature during convalescence generally meaning a relapse with extension of suppuration demanding immediate evacuation. (2) A rapid fall, and especially a rapid fall in the presence of an increased pulse rate. Such a combination means the almost hopeless stage of general septic poisoning. Handley remarks: "The pulse and temperature may be found normal in the presence of an abdominal disaster. An appendicitis of the most deadly character may commence merely with acute general abdominal pain, no other symptons appearing for six to eight hours, during which the pulse and temperature may remain normal." Again a careful record of the temperature in the intervals between attacks sometimes leads to interesting revelations. We meet with cases where the patient, having had an attack of appendicitis and recovered, is subsequently subject to what are called "bilious attacks," and will be found to have a varying temperature with probably a chronic ulcerative appendicitis.

The next point we will consider is,—

When to Operate.

(1) *Early Operation.*—Where the progress of the disease is arrested by surgery before the occurrence of various untoward complications, such as peritonitis, septicæmia, pylephlebitis, etc. The opinion of American surgeons is early operation from the first; French surgeons are now of the same opinion; Germans are more conservative and in favour of delay, thus agreeing with English surgeons. The ideal time for operation is within the first few hours, not later than the first twenty-four hours, before the formation of exudation, abscess or adhesions, and where the appendix can be readily detached from the surrounding tissues. (2) *Intermediate Operation*:—Say the patient is first seen on the second or third day of illness, the surgeon has then to consider the difficulties and dangers of breaking up fresh adhesions, or disturbing a localised infection; these some operators consider so great that they prefer to wait until a later period, in the hope that the disease will either abate or be absorbed, or else that a well-defined abscess be evacuated without danger—the case being watched from hour to hour. (3) *Late Operation*:—That is, one performed after the formation of a well-defined abscess shut off by adhesions, or else undertaken for a spreading appendicitis. (4) *Interval Operation*:—The interval operation is for one who has passed through one or more attacks, and for this reason decides to submit to the operation while enjoying apparent health, rather than incur the risk of another possibly fatal attack.

Possibly most of us have had experience of operating at all those different periods. It is generally difficult to get your patient to submit to "early operation," as he or she does not feel distinctly ill, but undoubtedly it is the ideal time for operation. As regards intermediate operation, I should play a "waiting game," watching the patient carefully and being prepared to "go in" at any moment; this, of course, is easy to do in hospital, but in private practice with your patient at home, or it may be far out in the country, you may have to operate at once. *Late Operation* is probably the condition we have to face most frequently. A goodly number of patients admitted to hospital are in the abscess stage, requiring immediate operation, and if the appendix is not found and removed at the time, then *Interval Operation* is advocated in order to have the offending organ removed.

ERRORS IN DIAGNOSIS.

The following local conditions may give rise to an error in diagnosis:—

(*a*) *A Tumour or Inflammatory Condition of the Right Ovary or Tube.*—When we consider the intimate relationship that exists between the blood, lymph and nerve supply of the right ovary and appendix, it is not to be wondered that mistakes in diagnosis have been made. You may also have the appendix in contact with the right ovary and Fallopian tube, and even a vaginal examination may not help you. I saw a very interesting case lately. A stout, middle-aged lady was seized with violent pain in the right iliac region; so intense was the pain that she would hardly allow any pressure to be made over that area. The pain had come on quite suddenly, and she had previously suffered from colicky pains in the abdomen, suggestive of appendicitis. The pulse and temperature were but slightly raised; vaginal and rectal examination was negative; she looked distressed but not really ill. Three days previously I had seen the case I mentioned of the female who had intense right iliac pain, and on whom we operated to find an appendix full of pus and just on the verge of bursting. The two cases seemed so similar that I advised immediate operation, but she would not consent. The dangers and results that might have accrued to the other case if she had not been operated on were unavailingly pointed out to her. A small dose of tincture of opium was administered and absolute starvation insisted on. Next day she was better and the following day perfectly well. Her menses had come on, and that possibly helped to accelerate the relief. I am not yet decided in my mind as to whether the case was one of acute ovaritis or appendicitis, or perhaps both. It is a fact clinically recognised that attacks of relapsing appendicitis in women are frequently determined by the onset of menstruation. This introduces a difficulty into the diagnosis, for if the attacks be slight, although painful and accommodated by insignificant pyrexia, the symptoms may easily be due to the congested state of the right ovary.

(*b*) *An Ectopic Gestation* may be confounded with appendicitis, as the following case will show. A woman was brought to hospital with a big swelling in the right iliac fossa; she had an acute attack of pain over that area three days before, and the swelling had gradually increased. Temperature and pulse were both slightly raised; the latter was hardly suggestive of an internal hæmorrhage. A vaginal examination showed the uterus to be somewhat enlarged, and there was the history of a "show" some days previously. I was not convinced that the case was one of suppurative appendicitis, although she had been sent to me as such. I made my incision at the outer border of the right rectus. The right iliac fossa was full of blood-clot, and a ruptured Fallopian tube was found.

(*c*) *Worms in the Intestinal Canal.*—This may simulate appendicitis in children, as the following case will show. A young Polish Jewess, aged 8 years, was sent to hospital under my care. On admission she looked ill, intestinal looking, I might say, with a dirty tongue, high temperature and rapid pulse. Pain was complained of over the region of the cæcum, and there was slight muscular rigidity over that area, but no tumour was to be felt. I thought the case was probably one of appendicitis. There being a history of obstinate constipation, I prescribed a soap and water enema, which was ineffectual, so I prescribed a mixture of sulphate of magnesia to be taken every four hours until the bowels acted, which did not happen for thirty-six hours, pulse and temperature still persisting; then, after a couple of good motions, she passed a round worm which I have no doubt was the cause of the illness as she improved immediately after.

(*d*) *A Meckel's Diverticulum.*—I assisted a surgeon who was about to remove an appendix. When he cut down he found what looked like that organ very much inflamed, but it turned out to be Meckel's diverticulum, as the appendix itself was discovered later. I have also seen appendices epiploicæ which might readily have been taken for the appendix.

(*e*) *A Floating Kidney.*—I was called in consultation to see a young man who had two distinct attacks of acute pain in the right iliac region. A definite tumour was to be felt, very tender on the slightest pressure. This tumour mass was supposed to have arisen within the preceding two or three days. A good deal of muscular rigidity was present with localised pain; no difficulty in micturition, and no blood in the urine. From the history he gave I agreed with his medical man that it was a case of appendicitis, and that the offending organ should be removed. At the operation the tumour turned out to be a floating kidney; the appendix was normal.

(*f*) *Tuberculosis of the Appendix.*—I have seen a case or two, but as there was general tuberculosis, I don't think they warrant further comment.

(*g*) *Cancer of the Appendix.*—I mention this case as it occurred in a middle-aged coloured man. I have seen very few cases of cancer of any organ in either male or female coloured patients, and I have never seen a case of cancer in an aboriginal South African native, male or female. In Asiatics I have seen a fair number of cases. This strikes me as a point of great interest, that cancer should apparently be absent in the South African aboriginal, rare among the coloured people, and common among the whites. Sarcomata among natives and coloured people I find fairly common, which, with syphilitic tumour masses simulating cancer, I have had frequently sent to hospital as cases of the latter disease.

Lockwood has drawn attention recently to *vermiform appendices which, although healthy-looking, on removal may be diseased.* In such cases, he asserts, the appendix may be full of bacteria with eroded mucous membrane; he also asserts that fæcal concretions are a source of great danger to the appendix when apparently healthy-looking. I had under my care a couple of patients who complained of pain over the region of the appendix with tenderness on pressure over that area, and who were subject to apparent attacks of appendicular colic, yet on removal of that organ, to the naked eye it looked apparently healthy. Both patients got well after removal of the organ and complained of no further discomfort, although at the time with the naked eye I could detect nothing wrong with that organ; possibly the microscope might have revealed a diseased condition of the mucous membrane.

That pain, tenderness on pressure, muscular rigidity, etc., over the seat of the appendix is generally associated with trouble in that organ we will not deny, but sometimes I think *colitis* by itself is too frequently overlooked, and that may be the cause of the trouble, as the following cases show:—I assisted a surgeon to remove the appendix of a middle-aged lady; the organ looked healthy to the naked eye. After its removal she continued to complain of pain over that area, so she was advised to go home for a change. While in Dublin she was seized with the old pain, and her friends took her to one of the hospitals, where her condition was diagnosed as appendicitis; she informed the surgeon that she had some months previously had that organ removed in Kimberley. Fortunately the surgeon who had operated on her happened to be in England at the time, and he was written to. He replied that the organ had been completely removed, and the stump invaginated in the cæcum. She was then treated for colitis and ultimately got well. I had another similar case under my own care, where the appendix was removed, and yet the patient had all the old pains and discomfort afterwards. I fancy those cases are mainly to be found in women who have suffered from chronic constipation, setting up a mucus colitis, localising itself principally in the cæcum. Such cases I have found benefit greatly by the old-fashioned infusion of senna pods, freshly made; this relieves the constipation and seems to act as a tonic to the intestines.

As regards the *incisions* for removal of the appendix, there are three I use:—First, the *clean-cut incision right down to the appendix.* This I employ when I am dealing with an abscess, or sometimes when I suspect there will be many adhesions; it gives more room than the grid-iron incision. When one employs it the patient must be kept in bed longer, so as to ensure good union and obviate the risk of hernia. Experience has taught me to be careful when separating an inflamed peritoneal surface, to tie all the bleeding points, otherwise a hæmatoma may form afterwards. It may so happen that one observes but little bleeding at the time of operation, but when your patient comes round from the anæsthetic the increased activity of the blood-vessels may cause a severe hæmorrhage. The second incision I use is *McBurney's or the grid-iron incision*—that is, separation of the muscular fibres as they run in their course. This is an excellent incision, as a hernia is not likely to develop so readily afterwards, but, as above-mentioned, I do not care for it in pus cases. The third incision is known as *Battle's incision.* I find it particularly useful in women when one wishes to examine the Fallopian tube and ovary on that side. An incision is made through the skin and subcutaneous tissues down to the sheath of the rectus; this section is made obliquely downwards midway between the anterior superior iliac spine and the umbilicus, is about 4 inches in length, and is placed so that it is equally distant above and below this line. The outer margin of the rectus sheath can now be easily defined. The inner margin of the wound is retracted about half an inch, and the anterior sheath of the rectus opened close to the retracted inner margin for the full extent of the incision. The rectus is now drawn inwards, being separated from the outer part of the sheath with a few touches of the knife; then the peritoneum is incised for an inch or more.

Pelvic Abscess.—In treating an abscess in connection with the appendix by operation two points are of great importance. In the first place, it is necessary to make the incision over the most projecting or superficial part of the abscess. In the second place, it is essential to provide for free drainage from the lowest part of the cavity. Supposing, therefore, you have a pelvic abscess which is well defined above the pubic region, and which can be felt bulging well into the vagina or rectum, the point then arises, where are you to make your incision? There is no doubt but when an abscess bursts spontaneously into the rectum or into the vagina, it drains very freely and closes more rapidly than it would do if an incision had been made through the abdominal wall. Some surgeons advocate and practise rectal drainage for pelvic abscesses, while others regard it as "not surgery." My own experience of rectal drainage has been unfortunate. I did it in two cases. The first was a young lad who was admitted to hospital with a pelvic abscess which was pointing into the rectum. I may mention this case was, I believe, a case of "traumatic appendicitis," as he dated the attack from a kick he received on the football field. I decided to operate *per rectum,* as the swelling was so pointed in that region. I evacuated a large quantity of foul-smelling pus and put in a drainage tube. The temperature and pulse fell to normal, and for several days he went on apparently satisfactorily. Then the temperature again commenced to rise, the rectal drainage had almost ceased, and I was rather puzzled at first to account for the relapse, but soon a localised abscess formed in the right iliac fossa, another in the left, and a third under the diaphragm. Each of those I had to open in turn, and a terrible fight I had to keep the boy alive; eventually, however, he recovered.

The second case was in a young girl who had an abscess pointing well into the rectum. I first passed a trocar and canula into the mass and got away some pus; then I made a small incision into the rectum, enlarging it by sinus forceps. Out came pus. I drained with a tube into the pus cavity, bringing the other end out at the anus. The child seemed relieved after the operation, but suddenly in the night she was seized with acute pain in the chest and fell back dead, the cause of death being probably a pulmonary embolus. I have ever since had a dread of opening those abscesses *per rectum,* as you cannot see or tell what you are doing, and you are near large venous trunks, which you may readily damage with your sinus forceps while stretching the wound.

C.—SECTION OF PUBLIC HEALTH.

I.—ÆSTIVAL DIARRHŒAS OF SOUTH AFRICA.

By G. D. Maynard, F.R.C.S. (Edin.), M.R.C.S. (Eng.),
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(Special Subject for Discussion.)

The magnitude of this subject, and the difficulty of obtaining satisfactory South African data—in sufficient quantity to enable reliable deductions to be drawn from them—was not fully realized until after I had accepted the kind invitation of your committee to open this discussion. Moreover, the term "æstival" does not limit the subject much, for most types of diarrhœal diseases, although occurring sporadically throughout the year, become epidemic or endemic in the spring.

It will perhaps be well to limit my observations to a few points, rather than attempt to cover too wide a field. In the following remarks I have attempted to show:—

1. The need for simplification in nomenclature and more uniform methods of registration.
2. The importance of the age factor.
3. That specific infection of food as a cause of these diseases has probably been overrated.
4. That lowering of the body resistance due to improper feeding, climatic changes, insanitary conditions, etc., is of prime importance in the ætiology of these diseases.

The first question to decide is, what sub-groups are we to include in the term diarrhœas? In the death returns from the various provinces and towns of South Africa we find the following headings:—Dysentery, Epidemic Diarrhœa, Zymotic Diarrhœa, Diarrhœa, Cholera Nostras, Enteritis, Gastro-enteritis, etc. No two reports adopt the same nomenclature or methods of returning these diseases. The position in England was until very recently little better, and is by no means perfect at present. In 1899 the Society of Medical Officers of Health appointed a committee to investigate this question. Medical Officers of Health were requested to inform the committee how they recorded diarrhœal diseases, and into which of the specified groups they entered the various diseases in making up their death returns. The replies showed that there were "165 different ways in which those causes of death were classified in the vital statistics of the Health Officers. If we had dealt with all the individual names in our analysis, that number would have been increased to an enormous extent."

Do these numerous terms really refer to separate diseases, or is it merely a question of the personal fancy of the certifying practitioner as to whether a case is returned as Enteritis, Zymotic Diarrhœa, Cholera Nostras, Gastro-enteritis, etc.? Again, what is the dividing-line between Diarrhœa and Dysentery? While the diagnosis in a case of classical dysentery may present no difficulties, many cases are seen in South Africa where an apparently simple diarrhœa develops dysenteric symptoms, and is ultimately indistinguishable from true dysentery. In discussing this subject Professor Delépine says: "It is almost impossible to avoid some confusion when one has to deal with diseases which it is difficult to define clearly either by means of *symptoms* and *lesions*, or by means of some *causal agent* capable of demonstration in the majority of cases. Diarrhœa is a symptom produced by a number of causes, and each one of these causes, according to its *intensity* of action or to the *state of the patient*, may produce several types of diarrhœa." "From descriptions I have been able to find in standard books of reference, it appears to me that such terms as English Cholera, Cholera Nostras, Cholera Infantum, Choleraic or Choleriform Diarrhœa, Septic Diarrhœa, Summer Diarrhœa, Infantile Diarrhœa, etc., must, generally speaking, be taken as synonyms of epidemic diarrhœa. To the morbid anatomist, epidemic diarrhœa is an acute gastro-enteritis." "Thus, although we have grown accustomed to designate by different names certain types of diarrhœal diseases, it is obvious that these various names do not indicate a reasonable belief in the existence of so many diseases, but relate rather to the *mode of occurrence of a single disease, or of a small group of closely allied diseases.*"

I have quoted at some length from this author because his remarks on this subject have the weight of authority, and are most important. The premature division of diarrhœal diseases under ill-defined headings, which cannot be justified on clinical or pathological grounds, is certain to lead to confusion, and will retard the study of these diseases.

Dr. Newsholme expresses a similar view when he says: "The consciousness of the fact that diarrhœa is the name of a symptom, and not of a disease, has caused practitioners in recent years to certify many deaths caused by epidemic diarrhœa under the heading 'Enteritis' or 'Gastro-enteritis.'" "The vitiating effect upon the statistics of diarrhœa from the above cause has been illustrated by Dr. L. Parkes."

Turning to the other side of the question, it is equally clear that infantile diarrhœas are not due to a single infective agent. Most intestinal bacteria have been, at one time or another, accused of being the *fons et origo mali.*

Professor Delépine, referring to the classical researches of Escherich on the *bacillus coli communis,* says: "The researches of that observer showed that although the *bacillus coli communis* is a constant inhabitant of the intestines, it is capable under certain circumstances of acquiring pathogenic properties." Hueppe, supported by Gilbert and Girode and others, stated that it was capable of producing cholera nostras. "Lesage, Macé and Simon, Cumston, etc., have found *bacillus coli communis* in a large proportion of the cases of infantile diarrhœa, which they have investigated, and believe that organism to be the cause, or the most important cause, of summer diarrhœa in children." Professor Delépine goes on to say: "It will be noticed, however, that the bacteria which have been suspected on good grounds of being the cause of outbreaks of summer diarrhœa belong chiefly to types of bacteria which are inhabitants of the alimentary canal." "The bacilli of the colon group appear to be most intimately connected with the epidemic diarrhœa, both of adults and of children."

The importance of these last two quotations will be seen when we come to consider the effects of meteorological conditions and dietetic errors in lowering the body resistance.

Klein found the *B. enteritidis sporogenes* frequently associated with infantile diarrhœa and cholera nostras. Newsholme, quoting Ballard, says: "The causes of infant diarrhœal mortality are multifarious." Metchnikoff considers that the *B. proteus* is a frequent cause of the green diarrhœa of infants, and reproduced the disease experimentally in monkeys. He also states that he frequently found this bacillus in the stools of healthy adults during August, September, and October, although the stools of the same persons had been previously examined during the cold weather with negative results.

The following quotations are from a most valuable publication, "Milk and the Public Health," U.S.A.: "It has been shown by Bucher in this country, and by Escherich of Germany, that some of the severest forms of infantile ileo-colitis are associated with streptococcic infections, and are probably dependent upon them." "Anaerobic spore-bearing micro-organisms, Flügge showed their importance in relation to infantile diarrhœa."

It would seem, therefore, that in the present state of bacteriological knowledge we are not justified in saying more than that many intestinal organisms may have a close connection with diarrhœal diseases. There is no evidence to connect any one organism with any special clinical type of diarrhœa or enteritis.

In the succeeding remarks the term diarrhœa includes the returns made under the various headings, Enteritis, Gastro-enteritis, Epidemic Diarrhœa, etc.; and when dealing with the disease in infants, the returns under the heading Dysentery are also included. It seemed that no useful purpose would be served by dealing with the various headings separately, when so much depends on the idiosyncrasy of the certifying physician as to which term is used. It would, I think, be well if Registrars and Medical Officers of Health would agree to adopt one term for all diarrhœal diseases in infants under two, until such time as further knowledge will enable us usefully to subdivide the group.

In addition to the bacteriology of diarrhœa, the age factor of the patient is of importance, and must be considered in some detail. In respect of age, diarrhœal diseases fall into three statistical groups, which may be termed respectively the infancy curve, the young adult curve, and the old-age curve. I regret that the South African statistics are not sufficiently complete to enable one to obtain entirely satisfactory resolutions from the data. Certain broad features are, however, evident, which are confirmed by the study of the returns from other countries. Thus the infantile curve starts abruptly shortly after birth, that is to say, the greatest liability to infantile diarrhœa occurs at birth or very shortly after, but rapidly decreases. The mean is at about the eleventh month, and 90 per cent. of the cases occur in the first two years of life, the remaining 10 per cent. being spread over the third, fourth and fifth years. Sixty-seven per cent. of the cases occur in the first year. The second distribution or young adult curve has a much larger range, but the area at both ends is small; 76 per cent. of all the cases fall between the twentieth and fiftieth years of life. The mean is at 36 years, while the mode or position of greatest frequency is at 30. The standard deviation is approximately 12.5 years. The old-age curve, which, for our present purpose, is comparatively unimportant, I do not propose to deal with here, many cases of diarrhœa in old people being merely a terminal symptom of some other disease.

It will be noticed that between the ages of 3 and 15 very few deaths from diarrhœal diseases occur. It is a period of almost complete immunity from death by this group of diseases. This period of immunity cannot be explained by freedom from possible infection. The explanation, I think, lies in personal immunity, a natural power of resistance which, apart from the effect of climate, feeding or sanitary conditions, is a function of age.

This question of susceptibility to many diseases varying with age is one of much importance, a proper understanding of which would be of great scientific interest and practical use. Mr. Udy Yule has attempted a solution of this problem, which, however, is to me unconvincing. . In the first place the curves he fits to his age-frequency distributions give low values for the co-efficient of "goodness of fit," and therefore if his distributions are sufficiently representative to permit of deductions being drawn from them, the chances are against his curves being suitable ones. On the physiological or pathological side of the question many objections might be raised; the proposed theory will not, I think, account for disease distributions which are well fitted by a J type of curve such as is given by infantile diarrhœa or the enteric curve of soldiers who go to India. That a real alteration in susceptibility at different ages does exist is, I think, capable of scientific proof.

The factors of importance which determine the onset and course of diarrhœal diseases may be discussed under two heads, strength of infection and the condition of the subject.

Assuming as proved, which I think we must, that most of the diarrhœal diseases are intimately connected with a bacterial infection, it is only reasonable to suppose that the number of the pathogenic bacteria present will, *cœteris paribus,* have an effect in determining the severity of the disease, whether this infection be introduced from without or developed within the body.

The introduction of germs from without is considered by most authors to occur chiefly by means of infected food, principally milk. There is, however, considerable difference of opinion as to whether this infection of milk occurs most commonly at the house of the consumer or at its source of production. If infection at the dairy were common, one would expect that many more epidemics of infantile diarrhœa traceable to milk would have been recorded. That this is a possible method of spreading the disease is shown by an epidemic which occurred in Manchester in 1894. Professor Delépine, who investigated this outbreak—where 160 children were infected by milk from one dairy—traced the infection to polluted water used on the farm. It is also probable that milk may become specifically infected in the house of the consumer, and that a certain number of cases occur in this way. That infection through milk is the common cause of the disease is, I think, open to grave doubt. This is a hypothesis which, if it is to receive kindly consideration, must be supported by strong evidence.

We have already seen that most, if not all, the organisms which from time to time have been accused of causing diarrhœal diseases are normal inhabitants of the intestinal tract; and unless we assume that these organisms are much more dangerous when introduced into the stomach than they are when present in the intestine, it is difficult to see why the introduction of a few more at one season of the year should have the baneful effect which is commonly attributed to them. If this were the explanation, one would expect that food most liable to contamination with the specific germs would be the food most associated with deaths from diarrhœa, but this is not so.

Many authors, notably Newsholme, have shown that children fed on condensed milk suffer more heavily from this disease than those fed on cow's milk. An opposite view, *viz.*, that condensed milk is a safer diet than cow's milk, is widely held in South Africa, but available figures do not lend any support to the suggestion. The bacterial contents of cow's milk is much heavier than that of condensed milk. Few samples of cow's milk are free from *B. coli communis*, streptococci, etc. Newsholme says: "It is evident, therefore, that condensed milk is a much less favourable medium for the multiplication of the bacteria contained in it than cow's milk." J. E. Sandilands finds that the number of organisms in condensed milk, even after incubation at 37 degrees, both diluted 1 in 10 and undiluted, cannot compare with the numbers found in cow's milk. Further, in the summary of the paper referred to he says: "The numbers of bacteria in preserved and natural cow's milk have no direct influence on the incidence of diarrhœa."

It may be objected that it is the variety, not the quantity, of bacteria that is the important factor, but there is ample evidence to show that while intestinal organisms are common in cow's milk, they are comparatively rare in condensed milk; and, further, if it is claimed that it is a special strain that is required, it is clear from what has been said that again the chances are in favour of its being found in cow's milk.

If infection by milk is not of primary importance, to what is the reported reduction in diarrhœal deaths, following the use of sterilized milk, due? In the first place it should be remembered that the results obtained in different towns have varied very considerably. The best results have been obtained where instruction to mothers has accompanied the distribution of the milk. Without denying that the use of sterilized milk may reduce the amount of infantile diarrhœa, one may feel doubtful as to whether the reported results really reflect a condition due to the use of sterilized or pasteurized milk. So many other factors are involved with which the statistics usually do not deal that great caution is needed in interpreting the returns. In the English urban districts the highest diarrhœal death-rate occurs amongst the children of careless and improvident parents, or of mothers who earn their own living. These are just the classes who cannot, or will not, take the trouble to obtain sterilized milk and carry out instructions. It is therefore evident that to compare the death-rate among the children of these latter classes with those of the former will result in very misleading conclusions. The infants of mothers who obtain pure milk are a selected class, and cannot be compared with other classes, nor with the general population. The use of sterilized milk will, of course, prevent such epidemics as the Manchester one referred to above, but such epidemics are apparently rare, and do not greatly influence the general death-rate.

The introduction of regulations to secure a purer milk supply have also been accredited with a reduction of deaths from infantile diarrhœa. A fall in death-rate from diarrhœal diseases has taken place in Washington, D.C., U.S.A., the commencement of the decline corresponding to the introduction of a Milk Law in 1895. The rates per 100,000 for five-yearly periods are as follows:—1880-84, 162; 1885-89, 168; 1890-94, 175; 1895-99, 135; 1900-04, 109; 1905-09, 93.

Without further evidence one must not conclude that the fall since 1895 was necessarily due to the Milk Law. The fall has been progressive, while the introduction of the law was sudden. Other cities have shown a similar decline where no alteration in the milk regulations have been made. In some cases it has been attributed to the installation of a water-borne sewage system, in others to road making, improvements of the dwelling of the poorer classes, etc. Improvement in the general sanitary conditions of most towns have been made during the last twenty years, which, quite apart from decreasing the infection of foodstuffs, will, I think, tend to reduce the diarrhœa death-rate.

In Durban, for instance, it is claimed, I understand, that a fall in infantile diarrhœa is due to a large extent to the beneficial results of road-making. If we take the five years' returns, 1904-1908, which are all I have at my disposal, it will be seen that the number of diarrhœal deaths to 1,000 births is as follows: 61, 45, 29, 41, 53. Unfortunately the returns for age groups are not given in the reports, but as the large bulk of diarrhœal deaths are infantile, the ratios will be much as shown, although the rates are not correct. I have mentioned these returns because they will exemplify the danger of drawing conclusions from small figures. The first three years of these returns would furnish to the uncritical observer overwhelming proof of the efficacy of whatever sanitary improvement was at the time being undertaken, but, unfortunately, the figures for 1907-1908 bring us back to a condition very nearly as bad as that obtaining in the first two years of the period. Thus, as far as these figures go, any real improvement in the diarrhœal death-rate in Durban is more imaginary than real.

With the present fashion of attributing so great a responsibility for the spread of infection to flies, I should lay myself open to criticism if I did not refer to this subject. Flies are an unmitigated nuisance, and no doubt a possible source of infection of food, but that they are, in civil life, the main or even important agents in the spread of such diseases as diarrhœa and enteric fever has yet to be proved. The case against the fly as carrier of infectious diseases has not, I think, been treated in a sufficiently critical or scientific spirit; perhaps prejudice has warped our judgment. To show that a fly placed on infected material can subsequently inoculate an agar plate is one thing; but it is necessary to do much more than this to establish the fly as an important causal agent in the spread of disease. I do not mean to imply that this is all the evidence there is against the fly, but great stress has been laid on these laboratory experiments, which, unless the fly possessed germicidal properties, need occasion no surprise.

Sandilands suggests that the seasonal incidence of diarrhœa coincides with and results from the seasonal prevalence of flies. It has, however, been pointed out that flies become more numerous in the house while the diarrhœa is declining. This seems to be admitted by Hewett, as he proposes an explanation for the phenomenon. After admitting that "the relation of flies to summer diarrhœa is practically based on epidemiological and other circumstantial evidence," he goes on to say: "An objection has been made to the idea that the house-fly is a carrier of the cause of summer diarrhœa, this objection being founded on the fact that at the end of the summer the fall in the fly curve follows the fall of the curve representing the diarrhœal mortality, the flies being still more numerous than they were earlier in the season when the curve was rising. In making such an objection it may be pointed out that we are not considering the numerical abundance of the flies in the houses only; it should be remembered that with a decline of temperature the activity of the flies, especially out of doors, which is important in this case, is not so great, even though their numbers may be considerable indoors. With the fall of the temperature, therefore, the possibility of their carrying the infection decreases without a necessarily corresponding decrease in their numbers and the diarrhœa. Pasteur's classical experiments on anthrax infection illustrate the same principle. A very important curve falls in consequence." The sophistry of the ancients could hardly match such arguments as these. Having started with the admission that the evidence is chiefly circumstantial, the unpleasant fact that the fall in the diarrhœa curve does not correspond to the fall in the fly curve is explained away by assuming that the flies are less active and therefore less dangerous. No proof of the assertion is attempted. Are flies less fond of feeding on filth or visiting milk in the autumn? One would have thought that such a fact as this would have caused one to reconsider the position very seriously, seeing that the fly theory is at best a hypothesis based on circumstantial evidence.

Sandilands says, however, "the infective matter thus conveyed to food is generally the excrement of some person suffering from the disease." If this is so, there is then usually no need for the flies to go outside the house to become infected, as in the dwellings of those most liable to diarrhœa the soiled napkins of infected infants are almost invariably left lying about the rooms.

Again, in this country there are two seasonal maxima for flies, and the maximum prevalence of diarrhœa corresponds to neither. The spring diarrhœas are beginning to decrease when the flies are still quite numerous, while the late summer recrudescence of flies is not associated with an increase in diarrhœal diseases. The small autumnal rise in the diarrhœa curve occurs after the flies have practically disappeared.

Nash also considers flies a common source of infection in infantile diarrhœa. His paper is one of the most careful contributions to the literature of this subject that I have seen. The evidence, however, is almost entirely circumstantial, and many difficulties in the way of accepting the theory propounded, *viz.*, that flies are the chief factor in the spread of summer diarrhœa, are not dealt with.

Before leaving the question of the house-fly, reference must be made to Ainsworth's paper. He has attempted to estimate the daily number of flies and show their relationship to enteric fever and infantile diarrhœa. It is on some such lines as these, I think, that the problem will eventually be settled. In districts where fly-proof houses are common the incidence of cases in such houses and in unprotected houses, if comparable districts could be dealt with, might throw some light on this subject.

Turning now to the other side of the question, *viz., the condition of the subject.* We have already seen that age is an important factor, some periods of life being almost free from diarrhœal diseases. This no doubt means that the natural resistance is sufficient to deal with any infection that may occur, and to prevent the intestinal bacteria from taking on a pathogenic rôle.

It is a well-recognised fact that lowered bodily condition predisposes to many diseases. We all know the increased risk of tuberculosis which follows an exhausted and debilitated state in the young adult; the effect of chill in determining the onset of pneumonia, and many other similar cases. An example, from our present point of view, and one which will be familiar to most South African practitioners, is the almost immediate effect of chill and dietetic indiscretions in determining the onset of recurrent dysentery. In this case the pathogenic organism is probably dormant, and the lowering of the resistance of the subject by chill, etc., is followed by fresh activity of the germ.

It does not seem unreasonable, then, to suppose that a child whose resistance is lowered by improper feeding may become the prey to bacilli already present in its intestinal tract. If this is so, we should expect that, *cæteris paribus,* the more unsuitable the feeding the greater the diarrhœal death-rate.

I am assuming throughout that ratio of deaths to the attacked is approximately constant, so that it is fair to assume the attack rate is low when the death-rate is low, and *vice versa.* I am not aware of any figures which would enable us to establish or refute this assumption, but in the absence of evidence on this point the above seems to me to be the most reasonable supposition.

The only really satisfactory figures dealing with the effect of feeding on the prevalence of infantile diarrhœa that I have come across, are those given by Newsholme in the Brighton reports. These figures are most interesting. To approximate the figures to rates so that the various age periods can be compared, I have converted *pro rata* the figures given for methods of feeding to correspond with the number of children born and surviving at each age period, and thus from the death returns obtained the rates for diarrhœal deaths in relation to different methods of feeding, assuming, of course, that the number of children returned as fed on the various foods is a fair random sample of the whole population. Unfortunately some of the groups contain very few cases, and little weight can therefore be placed on these results. The more reliable figures are printed in black type.

Approximate Death-rate per 100.

Method of Feeding.	Age of Child. 0—3 months.	4—6 months.	7—9 months.	10—12 months.
I.				
Suckled only ..	·862	·674	·000	·000
Suckled and farinaceous food ..	3·225	1·852	·000	·610
Suckled and cow's milk	9·091	·000	·000	·000
Suckled and condensed milk	14·286	·000	·000	·000
II.				
Cow's milk only ..	42·308	36·67	12·73	9·30
Cow's milk and farinaceous food ..	12·500	·000	6·06	7·70
III.				
Condensed milk only ..	15·38	69·57	60·00	26·92
Condensed milk and farinaceous food ..	·003	27·27	·000	5·88
IV.				
Farinaceous food, &c. ..	12·50	50·00	·000	·000

This table, in spite of its unavoidable defects due to sparsity of data, shows features of great interest. As pointed out by Dr. Newsholme, the death-rate in children fed on condensed milk only is much higher than in those fed on cow's milk only, in spite of the fact that cow's milk is so much richer in organisms than condensed milk. A feature of interest and importance to which I have seen no previous reference is the *lower* mortality of infants fed on cow's milk or condensed milk *plus* a farinaceous food as compared with those fed on cow's milk or condensed milk only.

Cow's milk is much poorer in carbohydrates than human milk, and the same is true of unsweetened condensed milk. Sweetened condensed milk contains an excess of carbohydrate, but cane-sugar is an admittedly unsuitable form for infant digestion. Cane-sugar is very prone to give rise to intestinal fermentation which lactose does not seem to do. The reduction of the death-rate apparently due to adding farinaceous food to the milk diet is, I think, instructive, as showing the effect of insufficient nutriment in lowering the child's resistance to diarrhœal diseases. Probably the dextrose in baked flower, artificial infants' foods, etc., supplies to a certain extent the additional carbohydrate required when cow's milk is substituted for human milk.

The lowering of resistance to diarrhœal infections brought about by improper feeding is a factor of the greatest importance in the ætiology of these diseases. There are numerous other causes of lowered resistance, notably climatic changes and insanitary conditions. The evil effects of inhaling bad smells in predisposing to disease have long been recognised. Where houses are crowded together, courtyards not paved, no water-carriage system of sewerage or proper scavenging of rubbish installed, there all the conditions for lowering the health of the inhabitants by want of fresh air exist.

The evil effect of back-to-back houses in increasing the diarrhœal death-rate is seen in the following table, showing the relative death-rates from diarrhœa in through and back-to-back houses, the death-rate in through houses in each instance being given as 100.

		Diarrhœa.
Salford, 1879-83	(A) Through houses	100
	23 % back-to-back houses	109
	56% ,, ,,	149
	(B) Through houses	100
	18 % back-to-back houses	120
	50 % ,, ,,	184
Shipley, 1887-92	Through houses	100
	Back-to-back houses	182
Bradford, 1890-92	Through houses	100
	Back-to-back houses	231
Thirteen towns	Through houses in comparable selected districts	100
	Back-to-back houses in comparable selected districts	139

These factors will act with especial virulence on the infants and small children. Thus, quite apart from any specific infection, we have the conditions predisposing to disease. To the foregoing should be added improperly constructed houses, particularly in respect to impervious foundations, whereby the ground air is driven into the house by every change of temperature or fall of rain, or drawn in by the use of fires. The geological structure of the soil and subsoil, the physical geography of the district, the height and variability of the subsoil water, etc., each have their effect on the health of the residents.

There is another way of considering this question. If the main factor in causing deaths from diarrhœal diseases is specific infection of milk or other food, then there is no *a priori* reason why other causes of death, *e.g.*, pneumonia and bronchitis, should be high in districts where diarrhœal diseases are prevalent, and *vice versa*. In a most interesting and instructive report on "Infant and Child Mortality," written by Dr. Newsholme, figures are given which enable us to discuss this point as it applies to the English counties.

Dr. Newsholme gives the correlation value of the death-rate from *all* causes under one month to the death-rate from diarrhœal diseases for 43 counties as $= .55 + .07$. Now this figure is not, to my mind, entirely satisfactory, for the following reasons: (1) No correction has been made for the spurious correlation introduced by correlating rates. (2) The deaths from diarrhœa occurring in the first month of life are comparatively few, and when only one year's returns are dealt with, liable to wide fluctuations. (3) In dealing with the whole county, variations in the proportion of rural to urban areas will introduce an error, because the death-rates are not alike in these districts. This source of error will be lessened by considering urban and rural areas separately. (4) By correlating all deaths with an included group, a further error will be introduced. If we correlate two variables, say $(x + y)$ with x, and even if x and y vary independently, $(x + y)$ is dependent on x and varies with it; thus a false correlation is introduced.

I have attempted in the following figures to remove some of the objections enumerated above:—The partial correlation coefficients for deaths from diarrhœa to deaths from *all other* diseases for standard population for children under one year dying in the urban districts of fifty counties is:

$${}_{P}\rho_{AD} = \cdot 8637 \pm \cdot 0067;$$

for diarrhœal deaths to deaths from atrophy, debility, marasmus, and convulsions, for same age period, districts, etc., is:

$${}_{P}\rho_{CD} = 7835 \pm \cdot 0109;$$

for diarrhœal deaths to deaths from pneumonia and bronchitis for same age period, districts, etc., is:

$${}_{P}\rho_{BD} = \cdot 8270 \pm \cdot 0086.^{*}$$

In spite of the fact that these values are based on the returns of a single year, they are, I think, instructive. Even assuming that the partial correlation coefficients are only approximate, their high value shows pretty definitely a close association between deaths from diarrhœa and the sum of other diseases, as well as with the two special groups. The latter were chosen so as to exclude as much as possible diseases which might be caused by infected food; the former (atrophy, debility, etc.) as a group likely to be influenced by defective feeding as opposed to infected food. It may, of course, be objected that the explanation of the high correlation is due to the fact that careless parents do not feed their children properly in the one case, subject them to unnecessary exposure in the second, and use contaminated milk in the third. I do not think the correlation values would be so high as they are if this were the real solution, and there is probably some other underlying factor connecting all these diseases. I believe this factor to be a lowered resistance to disease, brought about by improper feeding, and augmented by unhealthy surroundings.

The seasonal aspect of the disease is one of great importance, and has been considered almost entirely in its relationship to the life-history of the infective organisms while outside the body. The critical earth temperature of Ballard has received much attention in England, but has been shown not to hold true for South Africa. It is difficult to see what is the exact relationship between the 4-foot earth temperature and the air temperature on which must depend the temperature of milk, etc., in which the organisms can breed. The earth temperature reflects the air temperature, but at a later date some weeks of warm weather are required before the 4-foot earth temperature rises much in England. Thus, while conditions favourable for surface bacterial growth have been present for some time without result, the effect on the child's health is not developed immediately; and in a country like England, where the heat is not very great, the period necessary to lower the infant's resistance corresponds possibly with the time required to raise the 4-foot earth temperature to Ballard's critical point. In South Africa, the heat being greater, its effects are more rapid. On the high lands of this sub-continent

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[*] The corresponding total correlation coefficients of the death rate are:—

Diarrhœa, with all other diseases	$r_{AD} = \cdot 49 \pm \cdot 03$
.. ,, atrophy, &c.	$r_{CD} = \cdot 49 \pm \cdot 03$
,, ,, pneumonia and bronchitis	$r_{BD} = \cdot 47 \pm \cdot 03$

the great diurnal range is also of consequence. The sudden evening chill following a hot day causes congestion of the abdominal viscera, and is probably a predisposing cause of diarrhœal complaints.

Dr. Newsholme, dealing with the meteorological factor, gives two tables showing the effect of temperature and rainfall on infantile diarrhœa. The death-rates, mean air temperature and rainfall, are given for an eight-year period for the third quarter of the year for fourteen towns. He claims that increase of temperature is accompanied by an increased death-rate, while the reverse is true of the rainfall and death-rate.

The figures given in respect of temperature do not bear out this suggestion, for the correlation between diarrhœal death-rates and mean air temperature for these fourteen towns = — .0199 ± .1795, a value that must be considered as zero. There is here no evidence that small differences in air temperature have any effect on the diarrhœal death-rates. In South Africa, heat *per se* does not seem to be the determining factor; the diarrhœal diseases are most prevalent in October and November, while the greatest heat does not occur until January. If infection of food were the principal cause of these complaints, one would expect that the hotter the weather the greater their prevalence, for even the greatest advocate of the causal importance of infected milk will not suggest that its temperature will ever exceed the optimum for bacterial growth.

Rainfall varies in the fourteen English towns above referred to much more than temperature, and its effect is marked. The coefficient of correlation between diarrhœal deaths and rainfall is = — .4796 ± .1388, indicating that increased rainfall is in England associated with a drop in the diarrhœal death-rate I have elsewhere shown that a high rainfall diminishes the enteric fever in the Transvaal. The beneficial and pleasant effects of a good fall of rain during the hot weather is generally admitted. The variations in seasonal prevalence of diarrhœa and enteric fever in India, varying with the periods of heavy rains, is most instructive, and indicates that temperature is of less importance than rainfall.

The effects of climate, weather and season on health are of great importance and little understood. Lauder Brunton has shown in an interesting paper some of their results. He quotes Weir Mitchell on the effect of storm areas on geographical distribution of pain areas, showing how they are associated. The baneful effect of change of season is reflected in the folklore of Europe. The universal exhibition of domestic remedies to avoid the spring illnesses is well known. The early spring bleedings of our forefathers show the same belief in the dangers of this season. No one has practised medicine in England without becoming aware of the crop of minor ailments that accompany the spring and autumn changes of weather. The same thing is seen in South Africa. These traditions show that alterations in season have a distinct influence on health, and no doubt lower the resistance to disease.

In closing this paper, I feel that an apology is required for the very partial and inadequate way in which I have responded to your invitation to open a discussion on "Æstival Diarrhœas in South Africa." The paper deals very little with conditions peculiarly South African, but very early I discovered that South African statistics relating to these diseases would not stand a critical analysis. It seems better, therefore, not to attempt to draw conclusions from such data, as the results would probably be misleading, and possibly grave injustice would be done to certain towns whose only fault was better certification and registration. I have, however, ventured to draw your attention, firstly to the urgent need for better methods of registration and a uniformity of nomenclature, as this is a necessary preliminary to a satisfactory study of the diseases locally; and secondly, to the importance of proper feeding as a prophylactic measure. I must ask your pardon if I have overstated the case for endeavouring to establish the individual's immunity by keeping the general condition at the highest level, but I venture to think that this view, although by no means new, is one that has been lost sight of while the infective theory held the field.

Conclusions.

1. That in children under five years the diarrhœal diseases cannot be distinguished clinically or bacteriologically; no useful purpose, therefore, is served by continuing the use of the present complicated nomenclature.

2. That there is urgent need for uniformity in the registration of the diarrhœal group of diseases both as regards nomenclature and age-grouping.

3. That the incidence of these diseases at various ages is a function of the age, a fact which cannot reasonably be explained by assuming that certain age periods are free from liability to infection.

4. That the organisms which have been claimed as the infective agents in the diarrhœal diseases of infants are normal inhabitants of the intestinal tract.

5. That if infected milk is a common cause of these diseases, it is curious so few epidemics have been traced to this source.

6. That the case against flies as the cause of epidemic diarrhœa still lacks scientific proof.

7. That the foods most prone to bacterial infection are not those associated with the highest diarrhœal mortality.

8. That there is some evidence to show that the addition of farinaceous food to either cow's or condensed milk diets lowers the death-rate from diarrhœal diseases.

9. That one of the most important factors in the ætiology of infantile diarrhœa is a lowered resistance, caused by improper feeding, climatic changes being the determining cause of the appearance of the disease in epidemic form.

Discussion.

Dr. Boyd, after expressing appreciation of Dr. Maynard's very interesting paper, pointed out that the view that infantile diarrhœa is mainly due to improper feeding and other causes entailing a lowering of the resistance may be right, but he was inclined to think that Dr. Maynard had strained the argument in order to establish his conclusion.

With regard to the question of food infection, he did not agree with Dr. Maynard's views that the disease was chiefly caused by normal bacterial inhabitants of the intestines. Morgan's bacillus, for instance, is not a normal inhabitant of the human intestine; neither is the *Bacillus dysenteriæ,* which in America is regarded by many as a common cause of diarrhœa. His experience in Pretoria shewed that there was a very close seasonal correspondence between the prevalence of diarrhœa and dysentery. Personally he had not found much difference as regards incidence between children fed on cow's milk and those fed on condensed milk. Newsholme has pointed out that infection of milk usually occurs in the dwelling, and condensed milk is here very liable to infection. It has been shewn that undiluted condensed milk does not form a very suitable medium for the growth of most pathogenic organisms, but condensed milk is often kept after dilution, and in diluted condensed milk most bacteria grow rapidly. The use of such milk would, therefore, favour specific infection.

With regard to the fly hypothesis, he was inclined to regard it as "not proven." Even Niven has admitted that it is difficult to explain why the diarrhœal rate should decline prior to the disappearance of flies.

He agreed with Dr. Maynard that the explanation that flies were more sluggish in the latter part of the season is not a very satisfactory one.

He was inclined to attribute great importance to the *method of feeding* as opposed to the *nature of the food.* It is generally admitted that there is a greater tendency to the development of diarrhœa in infants fed from bottles with long indiarubber tubes than those fed from bottles having no tubes.

He had found that, in Pretoria, the rate for infants fed with long-tubed bottles was 160 per 1,000 as compared with 100 for infants fed with tubeless bottles. Where infants were fed partly at the breast and partly otherwise, the use of a spoon or cup was much safer than a feeding-bottle. He had found that in infants of 6 to 9 months the rate was only 2.8 per cent. in infants fed partly at the breast and partly with a spoon or cup, and 66.6 in infants fed at the breast and with a bottle. These facts point to the importance of infection rather than of unsuitable food.

Then again there was the very general belief that it is dangerous to wean children at certain seasons of the year. If the age factor plays such an important part in the incidence of infantile diarrhœa, how can the comparative immunity of the breast-fed infant be explained?

With regard to the advantages of a mixed milk and farinaceous diet, the figures quoted by Dr. Maynard were very small, and it further seemed to him that there was a lurking fallacy in them—they dealt only with the food, and not with the method of feeding. The very class that uses only condensed milk is also that which is most apt to use bottles with long tubes.

Then as regards the incidence of the disease in back-to-back houses. In Leeds it had been found that the incidence in such houses was less than that in "through" houses, so that the relationship is not a constant one. In back-to-back dwellings other considerations besides impure air and consequent lowered vital resistance came into operation; in such dwellings there was a special difficulty in disposing of fæcal matter and refuse, and thus a special liability to infection. It is not quite correct that in Pretoria the greatest diurnal range of temperature is in the spring. Sudden changes of temperature cannot be regarded as a very important cause of infantile diarrhœa; if they were, we should get a large crop of cases after a period of great diurnal variation.

As Dr. Maynard has previously shewn in a very valuable paper, the incidence of typhoid fever in this country is greatest when there is a certain amount of moisture and a certain amount of heat; heavy rainfalls do not ordinarily produce any very great increase of prevalence, at least of sporadic cases. In England, infantile diarrhœa is a disease of the summer; in the Transvaal it is a disease of the spring. This circumstance suggests an interesting point for investigation, namely, the contrast as regards season of maximum prevalence in different parts of South Africa as indicating the exact meteorological factors which are of importance.

Dr. Caporn pointed out that, in Cape Town, tubeless bottles were now used practically by all.

Dr. Hopkins mentioned that, in his experience, attacks of diarrhœa in infants frequently followed an attack of tonsilitis. He laid stress on the influence of improper clothing and polluted water in favouring the disease.

Dr. Tomory, while disagreeing with many of Dr. Maynard's conclusions, agreed with him as to the importance as a factor in the causation of the disease of the lowering of the vital resistance of the body. He emphasised the view that the great bulk of cases of infantile diarrhœa arise from infection, and that " the most painful cleanliness " was necessary to prevent such infection. He would like to know whether practitioners had noticed that children attacked were initially in a feeble state of health.

With regard to the question of flies, he pointed out that while it had not yet been proven that flies played an important part in the dissemination of the disease, it was equally true that the converse was also not proven. Improvement in general sanitation is associated *pari passu* with reduced death-rates from diarrhœa, dysentery and typhoid fever. He agreed with Dr. Maynard as to the desirability of improving the nomenclature of diarrhœal diseases. The term " dysentery " should be strictly reserved for diseases due to certain specific organisms.

Dr. Waldron stated that he had been much impressed by the fact that, in the majority of cases of infantile diarrhœa which had come under his notice, there had been an initial stage, before any definite illness had set in, during which the child's digestion was upset. In very few cases was there the definite onset which one ordinarily looks for in a specific infection. A useful, though unscientific, classification was to divide diarrhœa into inflammatory and non-inflammatory. He agreed with the previous speaker that the term " dysentery " should be reserved solely for diseases due to certain specific organisims.

Dr. Bensusan stated that he had not been accustomed to attach the importance to the lowering of the power of resistance that Dr. Maynard had done. If there was a lowering of vitality acting previous to the onset, there were many cases in which no such lowering could be detected by the physician. One of the arguments Dr. Maynard makes use of is the reduction of death-rate in mixed carbohydrate and milk-feeding, and that the dextrose contained therein supplies an additional carbohydrate required when cows' milk is substituted for human milk. He had invariably found that in cows' milk feeding the mother usually supplied the carbohydrate in the form of sucrose or lactose.

Dr. D. de V. Hugo stated that, in his experience,, infantile diarrhœa was not a disease of the spring; it was generally most prevalent after small showers following a period of hot weather. He was inclined to regard heat or sudden changes of temperature as playing a large part in determining the incidence of the disease. He drew attention to the frequency with which the breath of infants suffering from the disease smells of acetone.

Dr. Davey considered lowered vitality a common predisposing factor. He had also noticed in many cases that the breath had an odour resembling acetone or chloroform and suggestive of derangement of the liver. The disease, in his experience, was by no means confined to the poorer classes. Difficulty in treating cases with peptonised milk arose from the fact that peptonised milk is bitter, and the child frequently refuses to take it.

Dr. Porter called attention to the importance of Metchnikoff's observations regarding *B. proteus* as a cause of infantile diarrhœa, and the precautions he recommends to guard against such infection.

After further discussion, chiefly regarding nomenclature, Dr. J. A. Mitchell moved that the following resolution be submitted to Congress:—

> "That the attention of the Government be called to the great desirability of the early institution throughout the Union of a uniform nomenclature and system of Deaths Registration, and that in the drawing up of such a nomenclature one of the matters which should receive special consideration is the classification and grouping of the several forms of diarrhœal diseases."

Seconded by Dr. E. N. Thornton, and carried unanimously.

II.—THE SANITARY CONTROL OF MILK SUPPLIES.

By J. J. Boyd, M.D., D.P.H. (Camb.), Pretoria.

The necessity for the supply of pure milk being admitted, and the present highly unsatisfactory position being undeniable, the question to be considered to-day is, By what means can a pure supply be secured?

Methods of Control.

There are various methods by means of which the sale of milk is at present more or less imperfectly controlled. These are:—

(*a*) Inspection and analysis of milk offered for sale.

(*b*) Inspection of premises on which milk is produced or offered for sale, together with inspection of all vessels and utensils used in the business.
(*c*) Inspection of dairy cows.
(*d*) Inspection of persons engaged in the milk business.

Inspection and Analysis of Milk offered for Sale.

Unless milk is very foul indeed, containing, say, actual visible dirt such as fæcal matter or something nearly as repulsive, mere inspection by the unaided eye is not of much value in enabling us to determine whether it is fit for food.

It is of the utmost importance that some rapid method should be available whereby the soundness or otherwise of any given consignment of milk may be determined. Different tests have been proposed, *e.g.,* the temperature of the milk and the degree of acidity. All milk of which the temperature at the time of testing is found to be more that 50° F. is condemned and summarily destroyed. Such a regulation is both more necessary and more easily enforced in the U.S.A. than in South Africa.

On the one hand, ice in the States is cheap and easily obtainable, which is not the case here. On the other hand, the milk supply of American cities, obtained as it is from considerable distances, is frequently a long time in transit from the place of production to the consumer, thirty-six hours being not uncommon. In this country a large proportion of the milk is delivered to the consumer within three or four hours of milking. During such a short period there is comparatively little increase in the number of microbes, even at a high temperature. According to Ward* there is an actual decrease in the number during the first three hours after milking, even at blood-heat.

Where, however, milk is sent into a town by rail it may be safely assumed that a period of at least twelve hours has elapsed from the time of milking; and I consider that it would be justifiable to condemn such milk if found to have a temperature of more than 50° F. upon arrival in town.

Acidity.—Fresh milk has what is called an amphoteric reaction. In the great majority of cases milk which has been kept for some hours develops an acid reaction owing to the operation of the *B. acid. lactic.*

The time taken for milk to sour varies, as you know, (1) according to the temperature, and (2) according to the numbers of *B. acid. lactic.* originally present in the milk.

The temperature being allowed for, the degree of acidity of any sample will, therefore, be a guide as to the age and original cleanliness of the milk. It is now generally accepted that sour milk is by no means necessarily unfit for food. Notwithstanding this, it is clearly reasonable to forbid the sale of such milk as fresh milk. It appears, therefore, to be desirable that the sale of milk having a degree of acidity above a certain fixed standard should be made illegal, unless such milk is sold as "*sour milk.*"

Analysis of Milk.—The chemical analysis of milk has been systematically carried out for many years, and it is on the basis of the results so obtained that most regulations dealing with the purity of milk are framed. Bacteriological analyses are comparatively rarely made. Personally, I hold very strongly that this is a mistake, and that from a health point of view the bacteriological analysis is of greater importance than the chemical.

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* "Pure Milk and the Public Health."

Chemical analysis has for its chief object the detection of adulteration, that is, the addition of water or the removal of fat, and occasionally the detection of preservatives in the shape of added chemicals. However reprehensible and fraudulent, and to a certain degree injurious, such additions and subtractions may be, they are of infinitely less importance as regards the health of the consumers than the gross neglect which leads to the gratuitous addition of fæcal matter and filth of various kinds, and which opens the door to the entrance of disease-producing organisms.

The detection of preservatives by means of chemical analysis is undoubtedly of greater importance, not only because such substances are in themselves injurious, but even more on account of the indication which they give of the impurity of the milk which requires the addition of such preservatives.

Chemical analysis ought undoubtedly to be carried out, and the standard fixed for the solid constituents of milk should be a high one.

Bacteriological Analysis.—Within recent years the use of bacteriology in determining the purity, or otherwise, of milk has been considerably extended, but in my opinion such examinations should be much more frequently made. It is notorious that a great deal of disease is due to contaminated milk. Epidemics of typhoid fever, diphtheria, and scarlet fever have frequently arisen in this way. It is usually considered that contaminated milk frequently produces diarrhœa, and occasionally ptomaine poisoning. Tuberculosis, again, is undoubtedly caused by the consumption of specifically contaminated milk.

Except in the case of the last disease, bacteriological examination cannot be expected to reveal the presence of the actual disease-producing organism. But this does not greatly lessen the value of the process. A similar statement might be made regarding the corresponding examination of water, the practical value of which is universally admitted. The impurity of a water is held to be established when a given quantity contains certain organisms, *B. coli communis and streptococci.*

It is not the actual presence of these organisms that is the reason for its condemnation, but the indication which their presence gives that the particular water is subject to contamination, which may at any time become dangerous. In the case of milk, the fixing of a standard is much more difficult, for several reasons:—

1. Even the purest milk, when freshly drawn, usually contains a considerable number of organisms, many of which are streptococci, and although such streptococci from a healthy udder are harmless to the human subject, bacteriologists are not yet apparently able to distinguish them from dangerous pathogenic varieties.

2. Owing to the rapid increase of most organisms in milk kept above a certain temperature, a high bacterial count may not signify a particularly dirty milk, but may simply mean that the milk has been kept for a considerable time at a high temperature.

In the present state of bacteriological knowledge, to what extent can bacteriological examination help us in determining the purity of milk and its probable freedom from pathogenic organisms?

I must here emphasise the fact that I speak not as a bacteriologist, but as one charged with the practical application of bacteriological results.

The total number of organisms present in a standard quantity is now recognised to be of comparatively little value as a guide to the purity of water, and, owing to circumstances already mentioned, is of still less value in the case of milk.

As regards streptococci, the inability of the bacteriologist to distinguish between the harmless variety so frequently present in milk freshly drawn from a healthy udder makes their estimation also of but slight value. Houston, in a report for the L.C.C. (July, 1905), does indeed suggest a tentative standard—that the presence of streptococci in 1/10.000 c.c. indicates contamination—but points out the great possibility of fallacy.

The supposed association between outbreaks of sore-throat and the presence of certain special strains of streptococci in milk drawn from cows suffering from mastitis makes it of great importance to distinguish between the different varieties; the recent work of Savage, in England, leads one to hope that such differentiation may soon be possible.

B. coli communis.—Both Houston and Savage agree that the presence of this organism indicates outside contamination. It is never present in the healthy udder or teats. In the report already referred to, Houston says: "The *B. coli* test is a valuable means of measuring the degree of excremental pollution in milk, and also is a means of gauging its biological status, as regards *potential* danger to health." He suggests a standard under which all milk containing *B. coli communis* in 1/1,000 c.c. would be condemned. The reason for making the standard so very much lower than that applied to drinking water is the much greater probability of the *B. coli,* in the case of milk being non-human in origin.

It appears to me, however, that in cases where the test is applied to milk immediately after it is drawn from the cow, a much higher standard might reasonably be exacted.

In Pretoria, of fifty-six samples procured at the place of production within an hour or so of milking, *B. coli* was absent from 1/10 c.c. in 20, and absent from 1/100 c.c. in 33. In all but one case, in which *B. coli* was present in 1/100 c.c., or a smaller quantity, serious sanitary defects existed in the construction of the premises or the methods of conducting the business.

The fact that the *B. coli* increases rapidly in milk, the temperature of which is above 50° F., makes the quantitative estimation of this organism, in non-refrigerated milk, which has been kept for some hours, of little value in determining its *initial* purity, although it may still be useful as indicating its unfitness for consumption, owing to age and the temperature at which it has been kept.

Spores of Bacillus Enteritidis Sporogenes.—The estimation of these, according to Houston, is of particular value as a guide to the initial purity of milk, inasmuch as they show no tendency to increase. Houston has suggested that this organism should be absent from 1 c.c. of milk. My experience of this test in Pretoria appears to indicate that the standard is too low. Of 58 samples taken at dairies, cafés, and the railway station, only 4 showed the presence of these spores in as small a quantity as 1 c.c. In 39 instances this organism was absent from 10 c.c. In 15 it was present in 10 c.c.

In many of the cases where the proportion of coli was unduly high, and the sanitary conditions were proved to be very unsatisfactory, the milk showed no *B. enteritidis sporogenes* in 10 c.c.

In many American cities the dairy regulations include bacterial standards. These are apparently based on the total bacterial count, and although, according to Eastwood, in a recent report to the L.G.B., these standards are not strictly enforced, their mere existence and the frequent bacteriological examinations made have been of very great value.

Inspection of Dairy Premises and Utensils.—I do not propose to dwell in detail on the requirements, as regards structure of cowsheds and milkrooms, and the nature of milk vessels, etc., which should be insisted upon. It is admitted by all progressive dairymen that cowsheds should have impermeable floors and be well lighted and ventilated; that a special room—milkroom or dairy—should be provided for the purpose of bottling the milk and storing the milk utensils; that this room also should have an impermeable floor, should be well lighted and ventilated, and should be fly-proof and dustproof; a plentiful supply of pure water and suitable arrangements for cleansing and sterilising all utensils are also essential. Except in cases where the milk is delivered to the consumer within two or three hours of milking, it is further necessary that the dairyman should be provided with means for cooling the milk immediately it is drawn from the cow.

Besides insisting on the provision of the buildings and apparatus, mentioned above, we must, as health officers, lay down certain regulations regarding,—(1) the methods of milking in respect of the washing of the hands of the milker and the cleansing of the cow's udder; (2) the maintenance of the premises in a high state of cleanliness; (3) the methods to be adopted in straining and cooling the milk; and (4) the type of cans or bottles to be used and the method of cleansing and sterilising these.

The enforcement of regulations regarding structural arrangements is, in the case of dairies situated within municipal boundaries, a comparatively simple matter.

As regards the regulations which govern methods of milking and cleansing, there is very considerable difficulty in exercising sufficient supervision to ensure that they are carried out. The regulation—which obtains, I believe, in a number of places in this country—requiring the sterilisation with boiling water of all milk bottles and utensils is, in my experience, most difficult to enforce.

The process of sterilisation, involving as it does, in the case of glassware, a considerable loss from breakage, is strongly objected to even by dairymen who are otherwise careful and law-abiding.

These difficulties are felt even in the case of dairies within municipal boundaries. As regards dairy farms outside such boundaries, particularly those situated right in the country, effectual control, by means of inspection of the methods of milking, straining, cooling and sterilising of vessels is practically impossible. And here, particularly, comes in the necessity for regulations regarding temperature and bacterial content of milk.

This brings me to one of the questions which it is the object of this paper to elucidate. By what authority and in what manner is inspection of dairy premises supplying milk to urban districts, but situated outside the boundaries of such districts, to be carried out?

Unless there is such control, it is impossible and unjust to attempt to procure a high standard of sanitation in the intra-urban dairies.

In this country at the present time the law governing the sale of milk consists only of municipal bye-laws. Most of the larger towns possess such bye-laws. Some of these codes include a regulation dealing with milk brought from places outside the municipality. Thus, in the bye-laws of Kimberley and other towns in the Cape Province, Section 17 gives the local authority power to inspect extra-urban cowsheds from which milk is sent into the town, and where the conditions are found to be unsatisfactory, to prohibit the further sale of milk from such a source in the town.

In Pretoria, Sections 19 and 20 of the Dairy Bye-laws enact that milk produced outside the town and suburbs may only be sold through an agent in town holding a dairy licence and a permit to sell milk from any specified place outside. The permit is only granted on condition that the outside premises have been inspected and approved by a health officer as being up to the standard prescribed by the Pretoria Municipality.

A report to the English Local Government Board, by Dr. Eastwood, contains much interesting information on the methods in force in the U.S.A. for the control and improvement of the milk supply. In Massachusetts, the Local Government Board of Health of even the smallest town has power to exclude the milk of an infected or *simply dirty* dairy, whether that dairy be local or distant. In New York City there is a regulation that "no milk shall be received, held, kept, offered for sale or delivered in the City of New York without a permit in writing from the Board of Health and subject to the conditions thereof." Many other American towns possess similar regulations.

In New Zealand there is Government registration and inspection of all places throughout the country in which milk is produced for sale. The sanitary requirements are very high.

The objection to the present Pretoria Bye-laws, and to those current in some other South African towns, as far as this question of extra-urban dairies is concerned is the difficulty of arranging for inspection.

I should like to suggest that dairy farms sending milk into urban districts should be controlled as follows:—

1. No milk should be allowed to be brought into town from outside unless a permit has been granted by the Municipal Health Authority.

2. The conditions of the granting of such permit would be (*a*) that the dairy premises in question were open for inspection by such Municipal Health Officer; (*b*) that the premises and cattle had already been inspected and approved by medical and veterinary officers appointed by the Government; (*c*) that the arrangements made for the conveyance of the milk by rail or otherwise from the farm to the city were such as to ensure that the milk should be kept at a temperature not exceeding 50° F.; and (*d*) that all cases of disease occurring among persons on the farm or amongst the cattle should be forthwith reported to the Medical Officer of Health of the town or towns to which the milk was supplied.

In order to carry out such a system, it would be necessary for the Union Government to appoint travelling medical and veterinary inspectors, who would be required to visit and inspect such dairies periodically.

In the case of dairies situated within a radius of, say, six miles from the centre of a town, the Government might utilise the services of properly qualified municipal officers for such inspections. As most of the dairies are found within a short distance of the towns, this would greatly reduce the number of Government Officers required.

Inspection of Dairy Cows.—There can be no doubt that regular veterinary inspection is necessary, and such inspection should certainly be required at least once a year. In addition to this routine inspection, a regulation requiring the notification of sickness of any sort amongst dairy cattle should be rigidly enforced, and veterinary inspection would necessarily follow on this. The disease which chiefly calls for regular veterinary inspection of dairy cattle is tuberculosis. For the detection of this disease, the tuberculin test appears to be absolutely necessary, and this should constitute the chief item in the annual veterinary inspection. No milk should be allowed on the market except from cattle which have been subjected to the tuberculin test within twelve months, with negative results, unless such milk is pasteurised in accordance with special municipal regulations. As the prevention of the spread of tuberculosis amongst cattle is even more an agricultural question than a public health one, the expense of veterinary inspection and tuberculin testing should unquestionably be borne by the Central or Provincial Government, and not by the municipalities.

Inspection of Persons engaged in the Milk Business.—In most places where there are any bye-laws dealing with the milk trade, such bye-laws include one which requires that any case of infectious disease occurring in a person employed at the dairy shall immediately be reported. This is useful as far as it goes, but here again a difficulty arises with regard to places situated outside the municipality.

The owners of such premises should be required to report cases of infectious disease to some local medical man holding an official position, such as the District Surgeon, who should then telegraph the information to the Medical Officer of Health of the town to which such milk is sent.

Apart, however, from the existence of infectious disease, it appears highly desirable that milk regulations should include some dealing with the personal cleanliness and freedom from disease of the persons engaged in the trade, whether European or Coloured.

I have frequently found that some of the employees, and sometimes the proprietors of the business, were in a very unsatisfactory condition as regards cleanliness. Again, the risk involved in the handling of milk by persons suffering from tuberculosis must be serious.

In concluding this paper, I should like to say that my object in writing it is to focus attention on the most important points which should receive consideration in preparing the milk sections of a Public Health Act, and to endeavour to have such points thoroughly discussed prior to the framing of such an Act.

DISCUSSION.

DR. WALDRON referred to the extreme difficulty of supervising dairies in rural areas, and instanced the very unsatisfactory conditions which he found to exist ten years or so ago in the dairies in a rural area in the South of England, supplying milk to London.

MAJOR MCNAUGHT, R.A.M.C., referred to the extensive prevalence of Mediterranean fever in South Africa and the risks of the conveyance of the disease by goats' milk. He instanced from his own experience a case where the existence of the *Bacillus enteritidis sporogenes* was found in the milk supplied to a military hospital in South Africa; an inspection of the dairy revealed the existence of very unsatisfactory conditions.

DR. TOMORY dwelt on the risk of conveyance of tuberculosis and Malta fever by milk, and the importance of ensuring by means of the tuberculin test that dairy cattle are free from tuberculosis. He further referred to the necessity for scrupulous cleanliness in the handling of milk, and mentioned that he had devised a tap which also acted as a measure, and which would obviate the risk of pollution entailed by the process of measuring in the usual way. In view of the difficulties of ensuring that cleanliness is observed, he considered that the only safe course was to boil the milk before use.

DR. MACKENZIE dwelt on the necessity for better cleanliness in the carrying on of the dairy business, and also the need of boiling the milk before use. He said there were circumstances which went to shew that Malta fever can be conveyed otherwise than by milk.

DR. CAPORN mentioned that in one large dairy in Cape Town where steps were taken to apply the tuberculin test to the dairy cattle, 90 per cent. of them were found to be tubercular; there was, however, no reason to believe that any very considerable number of cases of tuberculosis had resulted from the consumption of milk from this dairy, a circumstance for which the practice of boiling the milk before use was no doubt largely responsible.

He urged the impossibility of securing a pure milk supply under existing conditions, and the advisability of establishing large Trusts to provide such a supply, or alternatively, that such a supply should be provided either by the municipality or by government.

DR. J. A. MITCHELL referred to the considerable number of outbreaks of enteric fever which had during recent years been caused in the Cape Province by infected milk supplies. In many of these the conditions, as regards ordinary cleanliness, found to exist in the dairies were very unsatisfactory. In one instance it had been the custom to wash the bottles with water mixed with sand, collected from a grossly polluted yard surface. He further pointed out that, in the Cape Colony, under the Public Health Act of 1897, municipalities had ample powers of framing regulations, both in respect of dairies within and without their areas. The difficulty was, however, the effective enforcement of regulations.

There was a great deal to be said for the practice of thoroughly boiling milk before use as being the only safe course; but nevertheless it was the duty of the authorities concerned to ensure that cowsheds and dairies are properly constructed and provided with a proper water supply and suitable facilities for the carying on of the business; also that in the storage, conveyance and sale of the milk due regard is had to cleanliness and the prevention of contamination.

Dr. Boyd, in reply to the points raised, referred to the risk of conveyance of Malta fever by milk and the experience of Major McNaught regarding the existence of the *Bacillus enteritidis sporogenes* in the milk from a dirty dairy. Boiling, he observed, destroys the germs but does not destroy toxines which have been formed in the milk before the boiling is carried out, and thus it results that milk may be just as dangerous boiled as unboiled.

There was good reason to believe that the milk of cows with healthy udders, but with tuberculosis in other parts, was capable of conveying the infection to man.

With regard to milk Trusts, he failed to see that much advantage would result from such an arrangement; in his experience the best results were generally obtained by small concerns where the proprietor looked after matters personally. Servants of a Trust would require as much inspection as dairymen do under present conditions.

The question of the establishment of a Government or municipal milk service carried one a considerable way into the realms of Socialism, and was hardly within the scope of practical politics at present. He knew of the regulations adoptable by municipalities under the Public Health Amendment Act of the Cape Colony, but pointed out that the existing law did not provide effective *machinery* for enforcement Such machinery should be supplied by the Government, which should provide the necessary Health Officers, Veterinary Surgeons and Sanitary Inspectors. This was one of the points he wished to emphasise; another was the necessity for routine bacteriological examinations of public milk supplies.

Dr. Porter emphasised the necessity for the Local Authority having power to refuse dairy business to unsuitable *persons*—analogous to the powers possessed by Licensing Boards in respect of liquor licences. He instanced a case which had occurred in Johannesburg, where, after three convictions for contraventions of bye-laws, the dairy licence of a very dirty and ignorant alien had been suspended by the Court for a period. His wife thereupon obtained a licence and carried on the business; she was three times convicted—on the last occasion for bottling milk in a bedroom in which there were several children with measles. Her licence was suspended, but her son obtained a licence and carried on the business, being twice convicted under the Regulations. By this time the father's period of suspension had expired and he was able to again obtain a licence. Thus the efforts of the Local Authority were rendered almost completely futile.

He alluded to the advantages of the card system for dairy inspections, under which "points" were awarded under various heads and healthy competition thus encouraged.

Dr. J. A. Mitchell pointed out that arbitrary powers to refuse dairy licences, without assigning any reason, would, owing to business and social jealousies and other circumstances, be liable to abuse, especially in the smaller urban areas; some effective safeguards would be necessary.

After further discussion, in which Drs. Boyd, Watkins-Pitchford, Tomory and Thornton took part.

Dr. Boyd moved that the following resolution be submitted to Congress:—

> "That Congress recommends that when new legislation regarding the sale of milk is introduced,
>
> (1) Powers be given to Local Authorities,—
>
> (*a*) To adopt a bacteriological standard for milk offered for sale; and
>
> (*b*) To refuse the issue of dairy licences to persons who are unsuitable on hygienic grounds.
>
> (2) That machinery be introduced for the efficient inspection and control of all farms producing milk for sale."

Seconded by Dr. E. N. Thornton and carried.

III.—NATIVE SYPHILIS IN THE NORTHERN DISTRICTS: ITS MENACE TO PUBLIC HEALTH.

By D. C. McArthur, M.R.C.S. (Eng.), L.R.C.P. (Lond.), Taungs,
and
E. N. Thornton, M.R.C.S. (Eng.), L.R.C.P. (Lond.), Cape Town.

Prevalence.—The concensus of Native opinion is that syphilis did not commence to make its appearance with rapidity and severity amongst the tribes until after the opening of the Diamond Fields, which drew continuously from this area for the ever-increasing labour requirements of the Kimberley Mines. The official reports, in connection with Warren's Expedition in 1885, show that the Natives were then badly infected, and the horribly diseased condition of the prisoners taken towards the close of the Langeberg Campaign, at Kuruman, is almost a matter of history. From that date onwards, until within quite recent years, the Annual Reports of District Surgeons presented to Parliament continually emphasise the fact of an ever-increasing prevalence of syphilis amongst the Natives of Bechuanaland and other northern districts. If we disregard the certainty of some earlier infection, and take the commencement of labour activity in the Diamond Fields at about 1874, there was, until 1906, when the dealing with the disease was placed directly under the Medical Officer of Health for the Colony, a period of over thirty years during which it had had an almost unchecked spread amongst the Native tribes of the area. Now, the Native population of Bechuanaland, excluding Gordonia, was, at the 1904 census, approximately 68,000. The annual number of passes issued to Natives for leaving these districts varies somewhat according to the seasons and to the requirements of Kimberley, but it is usually between 12,000 and 14,000. Women do not require passes to move from one district into another, so that no record exists as to the number of females leaving for work. It will be evident, however, from the number of passes issued, that a very large proportion of the male population, able to work, is absent for some months during the year at labour centres. More than four-fifths of the passes are issued for Kimberley.

In 1909, in the districts of Mafeking, Kuruman, Taungs and Vryburg, there were treated 2,367 cases of syphilis—or, in other words, 3.7 per cent. of the entire population of these districts received anti-syphilitic treatment. These figures do not take into account the large number of patients who have been relieved or cured in previous years, nor the large number of cases known to exist in outlying areas, which have not yet been brought under control. We do not think that we should be far out if we state that at least 5 per cent. of the entire Native population of Bechuanaland are suffering from active manifestations of syphilis, and that the percentage of those actually tainted with the disease is very considerably higher. In certain locations in the Vryburg District it is believed that as many as 25 per cent. of the inhabitants are afflicted.

Public Health Aspects.—Of the utmost importance in considering the effects of syphilis, when so widespread as in Bechuanaland, is to know in what manner the disease is affecting the birth and death rates. In 1907, 1,091 births and 1,162 deaths were registered as having occurred in the four districts of Kuruman, Mafeking, Vryburg and Taungs. In 1908, the births were 1,051 and the deaths 1,057; in 1909, 998 births and 706 deaths. It will be noticed that during the three years there has been a slight decrease in the number of births registered, but that the decrease in the number of deaths is much greater. At first glance it would seem as if the steps taken by the Government for dealing with syphilis in this area were actually reducing the death-rate; we know that they are, although, in our opinion, no deduction can be safely made from the statistics of births and deaths, owing to the faulty system of registration existing in the country districts of Bechuanaland, as in all Native districts of the Colony. However, such as the figures are, they show that in 1907 more deaths were registered than births, in 1908 the figures were about equal, and in 1909 the number of deaths had fallen considerably below the number of births registered.

It is, of course, useless to go into the causes of the deaths which occurred in the area during the years mentioned, as by far the greater number of deaths registered were uncertified, but in 1908 there was an undoubted increase in deaths from malaria. A large percentage of the total deaths appear, however, to have been due, directly or indirectly, to syphilis. Syphilis is seen to be causing a perceptibly lowered vitality of the race, which must lead to increased susceptibility as to tuberculosis and insanity, and also to want of stamina to resist the more ordinary ailments. Admitting the impossibility of stating the exact effect of syphilis on the birth and death rates, owing to lack of proper statistics, there is, nevertheless, abundant evidence that race degeneration is rapidly taking place in Bechuanaland. This applies parparticularly to the Native races properly belonging to Bechuanaland, such as the Batlapins, and, to a less extent, the Barolongs. It would seem probable that the future product of these races will be a Bantu degenerate of a similar but worse type to the mixed degenerate of the old coastal races, who may be found throughout the country to-day. The old Bechuana was not a man of stature, but he was a hardy, well-built individual. Of this there can be no doubt at all. To-day, it is the exception to see a fine adult specimen of either sex, and if one does, he has, as likely as not, lesions of syphilis about him. Also, mentally the Bechuana has shown remarkably little progress in development. These races have not suffered, as others have done, from the steady encroachment of European influences and civilisation. The majority still live under the direct control of their own chiefs, in their own large Native areas, reserved to them by the Imperial Government in the Act of Annexation. As a race they have had everything in their favour, under protective European government and guidance, and a marked racial improvement might reasonably have been expected. Yet it has been the reverse, and the explanation clearly is that of a steady syphilisation of the community. With the conditions existing as they are, there can be no question but that the continuance of the labour supply from the area is endangered. A race with syphilis rampant and already showing signs of degeneration cannot at any time be regarded as an ideal one from which to recruit Native labour, whether such be supplied to mining bodies or domestic service. Yet syphilis plus degeneracy is all that the Bechuanaland Native will be able to supply to the labour centres of this country, as far as can be at present foreseen, unless conditions materially alter.

So far as regards the male; but the Native woman must not be overlooked. She has been and will continue to be, an even greater danger to the various communities and services in which she seeks employment. Her sphere, of course, is chiefly domestic service, and that among the European communities. From personal knowledge one knows how often she goes to her service in a highly contagious condition. It therefore needs few words to emphasise the danger entailed to healthy families by the employment of such servants. Year by year instances of the contamination of innocent children through the media of Native and Coloured domestics accumulate, and yet the public remains indifferent.

Manner in which the Disease is Spread.—The whole question of sexual contagion is one which requires the most careful consideration, and depends mainly on whether we are dealing with the disease in its hereditary aspect chiefly, or as affecting the first generation. To uphold the view that the disease, as met with in Bechuanaland, is mainly acquired syphilis due to sexual contagion one must be prepared to admit that gross immorality has existed amongst the Natives, for by no other means could sexual contagion have produced the wholesale infection which exists to-day. The length of time the disease has existed would also have to be ignored. We are not of the opinion that sexual intercourse is of very much importance to-day in these districts in causing the spread of the disease from infected to noninfected persons amongst the country Natives. To establish the fact of sexual origin one requires at least a satisfactory proof as to the primary chancre on the genitals, but such is rarely obtainable. Primary chancres are certainly not common at all. Only thirty such cases have been seen out of 1,550 patients in the Taungs District, and of these, in eight only were all the facts in favour of the disease having originated through sexual intercourse. That the rarity of primary chancres is not a special feature of syphilis in the Taungs District is evident, for Dr. Beare, the District Surgeon of Kuruman, informs us that in his thirteen years' experience of syphilis in his district he has only observed what appeared to be primary chancres in about 1 per cent. of the Native cases treated by him, and of these one chancre was found on the buttock, and all the others were situated on the lips.

It is owing to the fact that the chancre seldom appears on the genitals, probably, that the Native pays so little heed to it, no more than he would to an ordinary sore, and it is the reason why so few primary cases of any sort are seen in practice in the infected districts. It is, of course, possible that this may hold good in many genital cases, but the Native is never found to attempt to hide any such trouble. Another point is important, and that is the infrequency of scars of old sores about the genitals of either sex. Moreover, in country districts, in spite of some prostitution of Native women with Europeans, but few primary chancres are met with in the latter.

The foregoing facts must be admitted as fairly strong evidence against any large amount of syphilis being spread by sexual intercourse in these country districts. Without doubt it is otherwise in the Towns, for the Town Native of either sex has every inducement to lose restraint, becomes usually thoroughly immoral, and, consequently, is frequently affected by sexual disease.

Experience shows that case-to-case infection without sexual intercourse is the method of infection which gives rise to the major portion of the acquired syphilis seen in these districts. The amount of direct evidence in support of this is very consderable, and cannot be disregarded. The reason for the disease being spread by this way is very largely due to the dirty habits and customs of the people. Amongst them it is usual for the pipe to travel from one to the other; their eating and drinking utensils are shared in common; clothing and bedding are treated in the same manner, and in many other domestic habits they are constantly risking contagion. The cases at Taungs with primary chancres show what fruitful sources of contagion are indicated. One is the son of respectable Coloured people, and was accustomed to play a mouth-organ with some Native boys, one of whom was under treatment for syphilis. In due course he became infected on the lip. Two other cases are young children, apparently with no family taint, who lived near a group of syphilitic patients, and became infected on the mouth. Another is the only child of very respectable Natives; she was a virgin, and no syphilitic taint could be discovered in either parent. She had a friend who suffered from extensive gummatous ulceration of the leg, possibly hereditary, but more probably tertiary. This friend used to visit her, and used to share the same blankets and clothes. Eventually she contracted a primary chancre in the upper femoral region. Another case, a male, infected a child, who, in turn, infected his mother. This is illustrative of the danger to Europeans from one another, after infection from a Native source, and is a sad and pitiable instance of the danger to which innocent families are, at present, being exposed through the condition of the Native population.

An important point in the liability to commensal infection is the character of the lesions commonly met with amongst the Native patients. The largest group, as one would expect, comprising about 35 per cent. of the cases, is the naso-pharyngeal. The danger of this group needs no emphasising. The striking feature, however, is the amount of affection of the cutaneous system. One sees it from the simpler syphilide to one with a more deeply ulcerative tendency. Rupia in its best-known form is not particularly frequent. It is usually replaced by a pustular syphilide, with a tendency to group in such parts as the buttocks, hips, knees, elbows, etc. How far this cutaneous class may be considered to be contagious is, perhaps, a matter of opinion, but, even if many of the manifestations be regarded as tertiary, one must not forget that latter-day research is proving how frequently the specific spirochæta is found in the tertiary lesions.

The group of exudative syphilides needs no comment as to their power of contagion. One cannot help being impressed by the above facts and the wide possibilities they offer, as regards commensal contagion. If such be not accepted, how do the numbers of acquired cases arise? As already stated, we cannot admit, from want of evidence, that they are usually of sexual origin in the country, but yet they are constantly occurring, and even the members of other tribes resident in this area, such as the Xosas and Basutos, are commencing to show signs of the disease in its acquired form. Taking into consideration the marked absence of any signs of primary lesions in the majority of patients, with, also, the probability of a very mild erosion of short duration, might it not be possible for the disease to be spread without a chancre at all? Well-authenticated cases of this nature have, at various times, been placed on record by Continental authorities, and it seems possible that in Native syphilis it might be that this is not uncommon.

It is, we think, probable that this difficulty in finding initial lesions has been the reason why the question of the disease being really yaws has again lately been raised. It is hard to understand how any similarity between the two diseases can be claimed. In no way do the symptoms coincide with those of yaws, as recognised elsewhere. There is no regularity of symptoms; there is not the regular age incidence, the itching, the peculiar smell, or the heaped-up crusts of eruption, and there are very few cases indeed which do not react promptly to mercury in some form. On the contrary, there is the primary sore in certain cases, the adentitis, the exanthem, the alopecia, the bone and eye affections, the polymorphism of the eruptions and the gummata, and, finally and conclusively, there are the hereditary features of the disease as we see them to-day in these Native districts.

When one comes to investigate the family history of patients, one finds a large amount of hereditary cases, and one has to realise that this phase of the disease is far more frequent than would appear from mere casual observation. It is important to gauge how far the hereditary taint has permeated the population and is answerable for the spread of the disease, for it is one of the most serious points to be considered in framing public health measures for the area.

Unfortunately, it is by no means easy to classify the two types in the cases as they present themselves. It appears to us that syphilis in Natives is by no means so typical as in Europeans. There are unusual variations in the manifestations. In many they are slowly progressing, in others almost quiescent. There is rarely the usual sequence of symptoms. Even in a typical primary case you are apt to get more than the usual overlapping of so-called secondary and tertiary symptoms. You also sometimes find apparently typical secondary and tertiary lesions showing themselves in the hereditary cases of all ages. You frequently see cases of late hereditary syphilis in young adults, presenting lesions often indistinguishable from those of the acquired type. Of course, one gets the snuffling babies with the many palpable signs of their taint, but there are many others of varying ages where doubt may at first arise. "Hutchinson" teeth are no more common than they are said to be elsewhere in tropical syphilis; iritis and interstitial keratitis occur, but they are not frequent phenomena in Native hereditary syphilis, as is experienced in Europe or India. There is, however, one particular manifestation which is fairly constant, and, when present, is, in our experience, typical of hereditary syphilis in Natives. This is the so-called "white head," so often seen in Bechuanaland. It is a pustular syphilide affecting the scalp. The pustules are isolated and scanty at first, but they gradually increase and tend to coalesce, forming thick whitish crusts of varying sizes. In some cases the head becomes covered, giving the appearance of a solid white cap. There is often a complete loss of hair, but in cured cases a partial re-growth takes places. It is, of course, an impetigo, but differs from the contagiosa in being non-inflammatory, much slower in growth, and the eruption is whitish instead of yellow. It is very intractable to treatment, liable to recur, and many of the cases develop naso-pharyngeal trouble. It is common in children and in young adults; this symptom constantly enables one to place them as late cases of hereditary syphilis. In our opinion, the condition known as late heredo-syphilis is a very important feature, as regards Native disease. In examinations of certain affected families, one finds some of the children showing no signs of disease. At a later period those same children usually come up for treatment with such symptoms as impetigo, commencing nasal ulceration, and the like. In these cases there can be no question of re-infection, and such cases must, we think, be regarded as evidence of late heredo-syphilis. There does not appear to be any marked tendency in most of these syphilised families to a lowered birthrate. A possible reason may be the absence of visceral and nervous lesions, as compared with European syphilis. This may have some effect in maintaining virility. At any rate, a Native can live and improve in health under conditons of active necrotic lesions which no European could exist with.

Existing Measures.—The law under which syphilis is dealt with in Cape Colony is contained in Part II of Act No. 39 of 1885. As will be seen later, the Act is not altogether applicable to the dealing with syphilis from a public health point of view. If effectual work is to be carried out, one must have some method of prompt notification of cases, and particularly there must be no default in the attendance of patients. The Act provides only for the District Surgeon to notify the Magistrate of cases, and though it does furnish some powers to deal with the attendance of patients, Magistrates, as a rule, are averse to prosecutions, on the score of the ignorance of the people, as to the nature of the disease, and also owing to a prejudice against filling their gaols with syphilitics.

Up till 1906 it was known that the disease was fairly prevalent in Bechuanaland, but its extent was not properly realised. In October, 1906, the Government, however, became aware of many of the terrible conditions prevailing, and it was at once decided to start systematic work to relieve the same. Taungs was selected as the most suitable centre for commencing the campaign, as this is a small but densely populated district.

At first a small reward was given to headmen and others for bringing in new and old cases for treatment. This was only partially successful, as the persons appointed could not be controlled in any way by the Government..

At last it was resolved to do away with the reward system, and two Special Constables were attached to the Native Inspector's Office. A scheme was drawn up by which all the patients could be kept in touch with by the constables, so that anyone who failed to attend after due warning could then be forthwith prosecuted. The work, of course, had to be placed on a strict official basis, and all directions and instructions had to come from the Magistrate's Office, in order to show the Native their official character. During the last two years the details of this scheme at Taungs have been gradually elaborated, and the scheme has proved generally successful. The defaulters' list, at present, out of an average monthly attendance of 550, has been reduced to from 2 to 3 per cent. The leakage of patients, who formerly used to get their passes at the Magistrate's Office, does not now easily occur, as reference to the books kept there shows whether any applicant is a patient under treatment or not. Should a patient get away without authority, it is discovered at the end of the month when the books are made up, and on his return proceedings are taken against him. The scheme commenced at Taungs has been gradually extended to the other infected areas in Bechuanaland, and though the work has not yet been properly co-ordinated between the different districts, there is no doubt but that it is meeting there also with a large measure of success.

As outdoor relief in the majority of infected districts is only afforded, and the patients have to come up considerable distances for their medicine monthly, treatment is necessarily of the simplest kind. The ordinary mercury and iodide solution, in supplies to last about three weeks, and hyd. *c.* cret. for infants, is the main line of treatment. Donovan's solution is also a useful adjunct for some skin cases. Besides these drugs, a certain amount of tonic treatment is often found to be necessary, especially in the worst cases, and necessary external or other medicaments are given freely. Taken as a whole, the reaction shown by the patients to the treatment given is most satisfactory, and some of the results have been surprising.

Defects in the Present System and their Remedy—It is now fully recognised by those who have to work the present Contagious Diseases Act how defective it is for the purpose of dealing with syphilis amongst Natives in areas such as Bechuanaland. Unfortunately, it is the innocent form of the disease which is doing such untold mischief in the Native Districts, and though the hereditary affection of children is one of the most important factors to be dealt with, it is here where the Act fails to be of assistance. In the matter of enquiry, the parent or guardian is mentioned, but when it comes to the penalty clause, no provision is made for inflicting any penalty on other than the person who fails to attend for treatment as required. This anomaly renders the Act inoperative where it is most urgently needed, and till it is rectified, a considerable amount of hereditary mischief, with active contagion, must be allowed to remain unchecked, for lack of suitable powers to deal with it.

Under the Act at present no person can be dealt with who is not reported to the Magistrate by the District Surgeon as suffering from syphilis. No one is compelled to notify suspected cases to the District Surgeon, and, consequently, he does not, therefore, become aware of the same, unless he comes actually into contact with them. It would be a good thing if some provision existed, to compel, at any rate in Native areas infected with syphilis, all headmen, parents, guardians, householders and employers of labour to notify the District Surgeon whenever a case of suspected syphilis is brought to their notice in their area, household or employ. We recognise that there are too many obvious objections to the disease being made a notifiable one in the ordinary way under the Public Health Acts, but still, some modified measure of notification is essential. Another amendment of the present law is needed, so as to enable any patient who cannot be properly treated as an out-patient to be compulsorily detained in a compound or hospital for the purpose of treatment. The law relating to the pass system should also be strengthened in these districts, and no Natives—men, women or children—should be enabled to leave their districts without passes obtained from the Magistrate or Native Inspector. Also, there should be provision for enforcing the medical examination of all Native labourers engaging on the mines, preferably by a Government Medical Officer, and labourers should be at all times under official inspection. It might be thought that, under the present arrangements at Kimberley, the disease would be discovered, but, as a matter of experience, this is not the case, and patients from all the infected districts are frequently lost, owing to their getting to Kimberley without authority. Some of these have been in a bad condition when they went, and are worse when they return, and there is no reason to doubt their statements that they have not been treated or medically examined while at work. Neither from Kimberley nor other labour centres have we ever heard of a case being returned by his employer as being unfit for work owing to syphilis.

A small hospital or compound in each badly infected district is, we consider, necessary. It is required for dealing with some of the worst cases, and particularly for those requiring surgical treatment. At the present time, in a few districts a hospital does exist, but in some it does not. In such a hospital one can employ remedies such as intramuscular injections, which one is not able to do on outdoor patients. The lack of co-operation between the officials of the different districts should be remedied and a uniform system laid down, with such modifications as local circumstances may require. This was gradually being brought about when, on the entering into Union, the Public Health Department of the Cape Colony was abolished. It would seem desirable, if a Union Public Health Department be formed, that an all-time Medical Officer should be appointed to co-ordinate the work of dealing with syphilis in these northern areas. If a patient is permitted to leave for another district, he should be transferred for treatment to the District Surgeon of such district. If he departs without authority, he should be traced and brought back for punishment, and for any further necessary treatment required. Moreover, it should be possible for the District Surgeon of one district to treat the patients from a neighbouring district who live nearer to him than to their own District Surgeon.

Now that Union is an accomplished fact, some of the older difficulties should disappear. For instance, patients frequently used to slip across to the Transvaal, where no such systematic attempt has been made to deal with the evil as on this side of the border. Most of the Natives working on adjacent Transvaal farms are tribally the same as those of Bechuanaland, and there is much intercourse between them and the latter Natives, and they are apparently also badly syphilised. Further north we have the inhabitants of the Protectorate, adjacent to Mafeking, and virtually of the same tribe. These people are also becoming infected to some extent, and uniform measures should be taken by the Protectorate Government to deal there with infected persons.

With the alteration in the Pass Law much could be done to prevent infected Natives leaving a District, but probably, also, something could be arranged at the point of destination. At the mines there would be a routine medical examination. At town locations, where visitors and new arrivals have to be registered, they could, if coming from an infected district, be made to submit to examination, so that, if infected, they could either be locally treated or sent back to their own districts with a notification to the authorities. The management of town locations generally, and of Natives in urban areas not living in locations, in these and neighbouring districts, should be improved.

Results and Future Prospects.—In recent years much has been urged by the advocates of mercurial injections and prolonged courses of inunction against the administration of mercury by the mouth, in the treatment of syphilis; indeed, oral administration is spoken of disparagingly by many Continental authorities, and by some English practitioners, such as Lambkin and Sir Felix Semon. This is not the place to deal with such a controversial subject; we would merely repeat that, in the treatment of syphilis in these Native Districts, most of the patients have received long courses of mercury by the mouth, with few bad or serious cases of mercurialisation, and our experience fully bears out the view held by English practitioners, that syphilis can be satisfactorily dealt with in this manner, particularly having regard to the strikingly rapid reaction of Natives to treatment. The plea is frequently advanced in South Africa that it is necesary to treat all cases of syphilis in hospital. We would say, " Treat all sexual cases in hospital, as well as the worst and most necessitous of other cases, but by far the bulk should, and can be treated satisfactorily as outdoor patients in their own districts, especially if administratve defects are remedied."

The results of the outdoor treatment in these districts have been most satisfactory. The cases have shown a general all-round improvement; many of the worst cases, who had been lying in their huts for years past, are now able to go about; men who have never been able to go out to work have been rendered fit to do so, and are now in a condition to go away for the purpose of earning money, without any danger to other communities.

If results such as these can be obtained only after a comparatively short period of special work, we may, we think, fairly regard them as a hopeful sign, in considering future prospects. The Government of the Union will doubtless realise the peril we are confronted with, in the North to-day, and will carry on the good work which has been done in the past. It will doubtless remedy the legal and administrative defects that have hitherto stood in the way. If it does so, can the evil now be arrested? We believe, as a result of our experience, emphatically that it can, even if we have to continue treating patients after the old methods. The recent discoveries, however, of new methods of diagnosis and of treatment lead us to hope that, in the course of time, the evil will not only be capable of being arrested but can be eradicated. In the future it does not seem unlikely that the serum diagnosis of syphilis, known as "Wassermann's reaction," may become of vast importance in dealing with syphilis in these northern races, though, admittedly, the process as known to-day is too complicated and tedious to be carried out as a practical measure. Think of what this aid to diagnosis may lead to if it can but be simplified. It will then be possible to have the serum of the relatives of every infected person, not showing obvious manifestations of syphilis, examined as a routine measure. Any patient from whom serum, which reacts, is taken will be at once placed under treatment, and the difficulty of knowing how to deal with the apparently healthy children of infected persons will be solved.

The new drug, known as "dioxydiamidoarsenobenzol," or Ehrlich-Hata's Preparation No 606, opens up possibilities of treatment hitherto unthought of. One injection of this preparation is stated to kill the syphilitic organism in from one to two, or sometimes five, days, and to prevent any recurrence. If this should prove to be true—and time alone can show this—the difficulty of dealing with syphilis in the North will disappear, for all that will be necessary will be for each patient, and for any who react to the serum test, to be injected with a solution of the new drug.

And if the serum diagnosis cannot be simplified so as to become a practical measure—but Ehrlich's remedy stands the test of time—we consider that, in order to eradicate the disease, the Government might, perhaps, be justified in seeking from the Legislature powers to order the injecting of the new drug into every member of the known syphilitic families in this infected area.

The Government of Cape Colony has done something in the past to hold the disease in check; it did the best it could with the means and funds at its disposal. Is it too much to hope that, whether Science has really at last given us a rapid and effectual remedy, or whether we still have to depend on old methods of treatment, the Government of the Union may seize the earliest opportunity of inaugurating a campaign which may lead to the arrest and ultimate eradication of the disease, not only in these districts, but in the other portions of the Union not so grievously afflicted?

The condition of these people is serious—a veritable menace to public health. If the menace existed for the Native alone, it would be bad enough, but could we even then, as medical men, sit still without trying to stop the evil? The disease is ruining what was a fine race—it is not owing to immorality that this is happening; it is working havoc amongst the children through no fault of their own; it will, unless more adequate steps are taken, ultimately make it necessary for the Natives of this area to be strictly confined to their own districts, and be debarred from supplying labour to the mines, when a few bad seasons would make them dependent on the State for relief. Unfortunately, it is not only a question of the native condition, but, as we have endeavoured to show, the jeopardy to Europeans, more especially their children, can no longer be hidden or ignored.

DISCUSSION.

DR. C. PORTER stated he would like to ask the authors of the paper whether they had any prison statistics of syphilis differentiating between primary and later cases; also as to their experience of intramuscular injections, and, finally, whether they agreed with the report of the Transvaal Government Commission on Contagious Diseases of 1906, of which Commission he was a member. As a result of its investigations, the Commission came to the conclusion that, as regards Transvaal Natives, syphilis had been introduced comparatively recently—in 1881 or shortly before. Its prevalence varied, at the date of the Commission, from "very little" to over 80 per cent. of the population (in parts of the Zoutpansberg). In mine Natives it was very low—only about 0.5 per cent. Primary syphilis is rarely seen; the disease among natives is not generally associated with sensuality, but "mediate" infection is very common. He mentioned that Dr. Arnold has devised an excellent system of inspection and notification of native syphilis in the Transvaal, and that Dr. Mekliss, of the Rietfontein Lazaretto, has recently treated a number of cases wth "606"; the results have been excellent—in some cases almost incredible.

The Commission recommended, *inter alia,* the dissemination among the natives of information regarding the disease, the examination of native labourers at the pass offices, and the provision of facilities for the treatment of cases, including the establishment of depôts for the distribution of antisyphilitic remedies, the intention being that issues of such remedies should be made only on medical prescription, to be renewed periodically.

DR. DE V. HUGO stated that this was a subject to which he had devoted special attention for ten years past, and he had formed the opinion that many cases of condylomata, usually regarded as syphilis, were yaws or a disease allied to yaws, which for convenience he would call "Y." He had diagnosed his first case of "Y"—a patient with anal condylomata—in October, 1909; the patient made a rapid recovery under treatment with iodoform externally. He had had a large number of similar cases since, and had repeatedly referred to the matter in his reports to Government as District Surgeon.

In contrast with true syphilis, in this disease (Y) the initial lesion, never a chancre, is a papilloma, identical in character with the later manifestations; there are no multiform eruptions, the hair is not shed, the internal organs and the glands—except perhaps those directly draining a papilloma—are not affected; there is no iritis, no scarring, excepting after ulcerations, no remote sequelæ, such as tabes or G.P.I.; the patients recover readily under treatment with simple external applications such as iodoform; they even recover in time without treatment of any kind, and their progeny show no distorted teeth or other manifestations of hereditary syphilis. " Y " is very contagious, and especially affects children, whereas syphilis is chiefly a disease of adults requiring usually special human contacts; he believed he had come across a few cases of re-infection by " Y." In both diseases the pains are worse at night; the lesions have a preference for the body orifices, and have a tendency to become circular: treatment by mercury and iodide suits either. He referred to a number of illustrative cases. In his own experience and that of others whom he had consulted, hereditary syphilis, locomotor ataxia and G.P.I. were practically unknown among the Coloured and Native population; if true syphilis were prevalent to that vast extent they were asked to believe, how could this circumstance be explained?

Dr. E. V. Jones stated that the class of case described by Dr. Hugo was not common in the North Western districts. Primary sores were not often seen, though he had seen three typical chancres during the past year. Natives, as a rule, take little notice of such sores. Typical tertiary lesions—gummata, periostitis, etc.—are frequently well marked, and the results of mercurial treatment are very satisfactory. Hereditary syphilis is not very common.

Dr. Martin remarked that the infection was frequently introduced, in his experience, by tattooing, or in cutting of tribal marks on the skin; he had known of Europeans being infected in this way. The disease is often mild in Natives; some treat themselves with copper sulphate.

Dr. Tomory also referred to the introduction of the infection by dirty knives used for cutting tribal marks and the comparative rarity of primary lesions and nervous sequelæ. He disagreed with Dr. Hugo's views as to the yaws or some similar disease. Not long before he had discussed the matter with a medical officer with West Indian experience; the latter stated that while at first sight some of the condylomata might be mistaken for yaws, careful observation made it clear that they were syphilitic.

Dr. Joubert stated that he had seen a number of cases of the type described by Dr. Hugo, but other members of the same families, often infected from the same source, had undoubted syphilis. As a rule eruptions were less marked in Natives; in half-castes, the whiter the skin the more definite the rash. The present law was inadequate, especially for sparsely populated areas. Cases in such areas should be removed to and treated in town.

Dr. H. A. Spencer stated that syphilis is very prevalent in his district (Middelburg, Transvaal). Primary sores are rarely seen except in the gaol or pass office. In cases of the yaws type there is always a history of syphilitic infection. The mines are the chief source of spread of the disease, and this source should be dealt with.

Dr. Thornton stated in reply that he wished Dr. McArthur had been present, as he had been mainly responsible for the paper, and would have been better able to reply to the various questions which had been put.

The prison statistics had been asked for, and he regretted that these were not available. Even if they were, they would be of little value in affording any indication as to the percentage proportion of cases of primary syphilis existing in syphilitics amongst the free population, owing to the fact that persons suffering from syphilis had been incarcerated for failing to attend under the Contagious Diseases Act, and, in selecting cases to be prosecuted, District Surgeons had naturally selected mainly old tertiary cases, which were not considered to be in a particularly infective condition. He did not consider it practicable, as stated in the paper, to treat Natives, when out-patients, by repeated intra-muscular injections, for Natives when once so treated would never come up for treatment again. He had read the report of the Transvaal Commission, but there was a good deal in it with which he could not agree, particularly the depot system. He considered it absolutely necessary for treatment to be given throughout by medical men, and deprecated any treatment by laymen. He thought that in some areas it would be useful to employ a whole-time medical man to travel round to treat patients, but as a general rule the Taungs system could be applied without difficulty, provided the officials were capable officers.

In regard to Dr Hugo's criticism, it was evident that no one else at the meeting held the views expressed by that gentleman, and he did not, therefore, propose to take up the time of those present by traversing these views, as they were really dealt with in the paper; but he might say that no single medical man who had had to deal with the disease in the North could not but deprecate their having been seriously brought forward, especially as there was a danger that the menace would not be properly recognised and coped with if it was thought by those in authority that there was doubt about the nature of the disease. In this connection, he reminded those present that the dealing with syphilis was no longer under a Medical Officer of Health, but was under the direction of laymen. After further remarks, he concluded by emphasising the menace, and by directing attention to the urgent need for legislative and administrative action.

Dr. H. A. Spencer moved that the following resolution be submitted to Congress:—

> "That Congress is of opinion that the prevalence of syphilis amongst Natives throughout the Union demands immediate attention, and that it should be one of the first duties of the Medical Officer of Health for the Union, on the organisation of a Union Health Department, to deal with it. Congress is convinced that this disease is not only a grave and increasing danger to both Europeans and Natives, but is operating as a powerful agent in the physical deterioration of the latter."

Seconded by Dr. E. N. Thornton and carried unanimously.

IV.—ON SOME CASES OF CUTANEOUS ANTHRAX INFECTION.

By F. T. Waldron, M.R.C.S. (Eng.), L.R.C.P. (Lond.), M.D. (Lond.), Mossel Bay.

Anthrax in any of its forms, so far as the human race is concerned, is apparently by no means common in this country, judging by the notification returns.

Having met with three cases of the cutaneous form of the disease during the last twelve months in the Mossel Bay district, one was prompted to make enquiries as to the occurrence of the disease elsewhere. During 1909 there were three cases reported in the whole of Cape Colony. In Natal, none. In the Transvaal the disease was made notifiable on March 1st, 1910, and during the four months ended June 30th, 1910, there were nine notifications of seven separate cases, all of cutaneous anthrax; three of them proved fatal. In the Orange Free State, none.

The cases being so few, it cannot be urged that the question bears at present any very great significance so far as public health and preventive medicine are concerned. Still, perhaps there is enough interest to justify a short note, as it is a matter that might conceivably become of importance in connection with one or two of our growing industries.

In some countries the subject has become serious enough to demand legislation for the better protection of workers with material liable to carry the infection.

It will be well first to give a short account of the cases met with.

The first was a white boy, aged 10 years, whose father is the owner of a tannery and boot factory. When first seen the patient had a collapsed bleb about the size of a shilling piece on the middle of the right cheek. It contained a little sanious fluid. Round the edge of the bleb, which was of a dark colour, was a ring of small vesicles, the whole lying on and being surrounded by a markedly indurated area of tissue. The whole side of the face was greatly swollen, but by touch the markedly indurated part could be sharply defined from the surrounding merely œdematous tissue. The preauricular gland was markedly enlarged, and the right eye was closed up by œdema of the lids. The temperature was 103° F., and the boy felt and looked ill.

The history given by the father was that an itching pimple formed first four days before, and that this rapidly changed into what looked like a blood blister. The marked swelling had come on rather rapidly in the last twenty-four hours. On being questioned as to the possibility of the boy coming into contact with any infected material, it was admitted that he frequently played amongst the dry raw hides and handled them. Nothing could be ascertained as to any one particular hide being responsible. The hides for tanning come chiefly from farmers in the surrounding districts.

Anthrax bacilli were readily found in the fluid in the central bleb.

The markedly indurated tissue carrying the bleb and ring of vesicles was excised with the knife, the depth representing about half the thickness of the cheek. The raw surface was freely cauterised with Pacquelin's cautery. It was dressed with frequent boracic fomentations. Recovery was uneventful.

The second case seen was in a young white farmer, aged 20 years. When seen he had an almost exactly similar local lesion to the last, except that the surrounding ring of vesicles, although present, was not so well marked. In this case the infection was on the front of the forearm, about midway between elbow and wrist. The area of induration was well marked, and was to be sharply defined by touch from the surrounding œdematous parts. This inflammatory œdema affected the arm from the tip of the fingers nearly to the shoulder.

The gland over the internal condyle was enlarged. The patient felt ill and weak, and the temperature was 102° F. As in the first case, it had begun four days before as an itching pimple, which then quickly turned into a bleb.

The treatment was again excision, but as the Pacquelin's cautery was not available at the time, the surface was wiped over with strong zinc chloride solution. Boric bath and boric fomentations were used, and recovery was rapid.

The history of infection was that two days before the pimple developed the patient had skinned one of his own oxen that had died rather rapidly and unexpectedly.

The only idea as to the cause of the death of the ox was that it might have strained itself.

The third case was a coloured man aged 22 years. Ten days before he had been driving a couple of oxen hauling stones.

One ox had died quite unexpectedly in the afternoon, after being apparently well the same morning. The patient skinned the dead beast. Two days later he had an itching pimple on the skin in the hollow below the lower lip. When seen he had the typical bleb, surrounded by a ring of vesicles, the whole lying on an area of marked induration. The whole of the lower half of the face and the neck were greatly swollen. The gland under the middle of the jaw was very markedly enlarged.

The temperature, 102° F. Anthrax rods were easily seen in the fluid from the bleb. It was the eighth day since the pustule had begun as an itching pimple.

A piece of tissue about the size of a half-crown piece was excised, representing the part felt to be markedly indurated, and on which the bleb and vesicles lay. Cauterisation was done again with the zinc chloride solution. Boric fomentations were afterwards applied, and the patient quickly recovered.

In addition to these three definite cases, I have seen two or three in which there was a local infection somewhat resembling anthrax, but not typical in appearance. In these a blood-stained bleb was present, but no ring of vesicles. Also the area of induration was not so well defined, although present. Each of these infections had taken place on the back of the hand, and the œdematous swelling affected the whole arm up to the shoulder. In two of these the anthrax bacillus was not looked for; in the third it was looked for rather hurriedly and not found. These three cases were treated with frequent boric baths and boric fomentations, and in each a small slough, skin-deep, separated at the site of the original bleb, and the great œdema of the hand and arm gradually subsided without any incising.

These were believed not to be cases of anthrax, but of some form of infection received from dead carcasses, causing a marked inflammatory œdema, not leading to pus formation.

In one of these three cases the history of infection was interesting. He was a coloured farm labourer. His master had just had thirty cattle inoculated against redwater.

The inoculation was carried out by a neighbouring farmer, who inoculated from one of his oxen. Twenty-five of the cattle died within three or four days of the inoculation. In skinning these beasts the patient received his infection on the hand. Having heard that a veterinary surgeon had been called in by the owner of the cattle to give an opinion on the catastrophe, I enquired as to whether anthrax septicæmia might have been the cause, but was answered in the negative.

At a rather hurried examination of a smear I could not detect any anthrax rods in the fluid from the bleb on the man's hand.

It is also, I believe, a fact that, liable as oxen are to infection with anthrax by the alimentary route, it is by no means easy to infect them by subcutaneous injection.

The last definite anthrax case seen, the man with the pustule on the chin, was infected from a beast that had been grazing on the same farm as these cattle had died on three months previously.

This is not mentioned as throwing any doubt on the correctness of the negative opinion as to anthrax in the cattle dead of inoculation, but the fact is given as it occurred.

Goats, sheep, cattle, horses, mules and pigs are all liable to anthrax. The acute form may prove very rapidly fatal, the beast dying in a few hours. In the horse and ox it may prove fatal more slowly in three, four or five days with gastro-intestinal symptoms. A cutaneous form is occasionally seen in the horse and ox, somewhat like malignant pustule in man. This may perhaps be due to infection carried by biting flies, according to veterinary opinions.

Through the courtesy of the various chief veterinary surgeons I am able to give some particiulars as to the occurrence of the disease amongst animals in the province.

Cape Province.—The disease is very prevalent in some districts, chiefly attacking horses and cattle. The parts in which it is most prevalent are the coastal districts of the Eastern Province, the Transkeian territories, Griqualand West, and parts of Bechuanaland.

Transvaal.—For the twelve months ending June 30th last thirty-nine cases were notified, nearly all in cattle. There were no cases in horses.

Orange Free State.—Not at all prevalent. During the last two years there have been only two small outbreaks, and those affecting only a few animals.

Cattle, horses, and mules have been affected.

Natal.—No epidemic has occurred, but sporadic cases have been met with in cattle in various parts of Natal and Zululand.

Preventive inoculation with an attenuated virus is occasionally carried out with success in this country for the prevention of the spread of the disease amongst cattle.

It is evident that the slaughtering and skinning of any single one of the many animals that die of anthrax is a possible source of danger. Unless an animal, such as an ox, is definitely known to have died of anthrax, and instructions given accordingly as to the burying of the unopened carcass with lime, it is pretty certain the hide will be removed, and probably the flesh will be eaten. A hide, salted, fetches about 6d. per lb., and means from 15s. to 20s. to the farmer.

There is no reason to suppose that the drying and salting process removes infection, as the spore form of anthrax is known to be markedy resistant. The eating of the flesh presumably would not be likely to do much harm if the cooking was as complete as it usually is. Both the veterinary and public health authorities are agreed that from time to time deaths take place amongst Natives probably from eating (practically raw) the flesh of beasts dead of anthrax.

The district in which these three cases of cutaneous anthrax have been recently met with is not, so far as I know, one of those looked upon as infected from the veterinary point of view. The probability is that a few chance cases in cattle happen, by a curious coincidence, to have given rise to infection in human beings. It is obviously not only directly, at the time of skinning, that the liability to human infection occurs, but at any stage of the exporting here or importing elsewhere of the hides. I can recall having seen about five cases of cutaneous infection at the London Hospital, all in dock labourers.

So far as I can remember, it was Russian hides that had there the reputation of carrying infection.

In the cases I have notified to our Divisional Council precautions have been taken to get the skins and carcasses destroyed to prevent further infection.

It is clear that anthrax being such a rapidly fatal disease in its commonest form amongst animals, the observation of any such involuntary inoculation experiments on human beings as those recorded might easily be a means of attracting attention to otherwise imperfectly explained death in beasts. Steps would then naturally be taken to prevent an infected hide travelling about and possibly causing further damage. The farmer seems to be ready to believe that an animal "must hurt itself" in some undefined way if it dies quite unexpectedly.

There does not seem to be any doubt that the human being offers a good deal of resistance to the generalisation of the anthrax organism in the system. The probability is that many malignant pustules would slough out unaided, or under simple dressings. There seems to be less risk of serious results if the infection is on the forearm or hand as compared with the risks of pustules on the neck or face.

From the results, in the three cases quoted, of excision carried out rather sparingly, there hardly seems to be any necessity for the removal of large areas of tissue such as I at any rate can recall as being the usual practice at the London. The removal of a large area of tissue and subsequent cauterisation of the raw surface seems in itself likely to cause a good deal of shock.

On the other hand, it is certainly very unsafe to leave the pustule to slough out by itself, unless perhaps when it is situated on such a place as the hand.

The fairly rapid growth of the hide-exporting industry of this country, and also of the local tanning, taken with the undoubted fact that anthrax is fairly common amongst animals, make it advisable for everyone possibly concerned to keep a sharp lookout for any infected material. Luckily anthrax is evidently uncommon in sheep, so that the far more lucrative and important wool trade here apparently remains free of reproach in that respect.

DISCUSSION.

DR. WATKINS-PITCHFORD pointed out that anthrax in animals was common in Natal. He knew of one instance where some fifty sheep were infected from one case in a sheep, and of another where four persons, including one European, had been infected from a case in a horse.

Dr. Caporn enquired whether the infection of an employee with anthrax would constitute an "accident" under the Employers' Liability Act.

Dr. Waldron replied that the Cape Colony Act did not apply to farmers. From the medical point of view, he would certainly consider such infection an "accident" within the meaning of the Act.

V.—THE CITIZEN'S CASE FOR A HEALTH DEPARTMENT.

By J. Barcroft Anderson, M.D., B.Ch., D.P.H., (Camb.),
East London.

The present seems to be a fitting time to discuss the question of the service which our special knowledge can place at the disposal of the ordinary citizen for the safe-guarding of his health, since now we are forming a fresh State, by the amalgamation of four States, each of which had traditions and laws which were in many essential respects dissimilar to each other; and therefore we are likely to look more to knowledge than to tradition for guidance.

Our profession habituates itself to placing the interests of those it serves before its own. Therefore I need offer no apology if I ask you to consider this question chiefly from the point of view of the private citizen. I will only make reference to our own interests by way of contrast when they may possibly be opposed to that course of action which we as a profession would recommend the public to adopt.

I also conceive that there is some obligation upon the citizens as a whole to express an intelligent desire in the matter before they, or we, can reasonably expect the State to take action. Hence the need that they should have an intelligent understanding of the case.

We are naturally inclined to assume that the public are aware of the extent of our special knowledge, and aware of the greatness of the change in their lives which would result from fully availing themselves of that knowledge, and of its expert application. A moment's consideration, however, is sufficient to convince one that any such assumption is wrong. Not one of ourselves has brought our knowledge of the advances in every branch of our profession up to date. And so much of our knowledge as is common to us all is of such a complicated nature—and is such an aggregation of small details which cannot always be summed up into great principles, where a little bit of the manifold mysteries of nature is revealed here, and a little there, with frequently the practical application of this knowledge not fully worked out—that only one possessing the whole of this fragmentary knowledge can form any just idea of its possible extent, of its general application in guarding against the many known risks to human health; or can fully appreciate the uselessness of anyone who lacks this knowledge attempting by administrative methods to induce the public to submit to restrictions on their lives which would be definitely for their benefit, but the significance of all of which neither the administrator nor the administered would then fully understand. And we alone know the possibility of advancement that opens before the public through the skilled indication of the direction and means whereby new remedies and new safeguards will be found. Hence the necessity there is, if we would further the public weal, that we should freely explain and advertise the nature and efficacy of the benefits that through our knowledge we can render the citizens of to-day.

One of the most extraordinary features of the relation of medical knowledge to the ordinary individual is that, though we need no advertisement to ensure his utilizing our services when ill, it devolves upon us to habitually keep before his mind the infinitely greater, though less remunerative, services which he can obtain from us towards retaining his health.

One can offer here only the most crude outline of the preventive work of the Health Officer. It may be conveniently divided into efforts to avoid the individual coming in contact with the infection of disease, and into efforts to maintain his health in such a good state as to render his body able to resist the infection of disease. Yet in most efforts to gain both of these ends, the same means must be employed.

The admission of plenty of sunlight to a room, its efficient ventilation, the dryness of its walls, ceiling and floor, all tend to destroy any chance infection which may be there, and apart from that, tend to the health of the occupier, and are therefore matters which operate in both the ways I have indicated. Therefore reason demands that, when structures for human habitation are erected, they shall conform to the known requirements of human health. Accordingly, the control of the details of construction of new buildings is by no means the least important of the various duties of a local Health Officer, and is elsewhere a matter which is usually subject to the most minute and comprehensive regulations authorised by the central Government.

It has also been found that human health and the risk of infection is affected by the width of streets, the height of houses in relation to the width of streets, by the existence of open spaces such as parks, rivers, lakes or commonage, by the location of sources of pollution of the air in the lee of prevailing winds, and by many other such things. Therefore, when a new house is constructed, it is of importance, not only that the building itself should be of sanitary design, but also that its position with respect to other houses should be such as to conduce to the greatest good of all. This is covered by the term " town planning," and when scientifically carried out, as has recently been extensively done in Germany, produces conditions congenial to human health and comfort beyond those existing in the older cities, which grew up through many generations, and whose inhabitants had no other thought of betterment than the production of the æsthetic when the gratification of the rich demanded it.

But the Health Officer must also watch for infection itself, and must watch all known channels whence it may possibly reach susceptible persons. Therefore he must be the ultimate authority upon the disposal of excreta and other refuse, and must supervise, prior to their consumption, all foods, drinks, and water, for through these are conveyed many diseases which together account for perhaps a third of our total deaths. He must also isolate all infected persons; yet as our intimate knowledge of the mode of infection in each disease increases, our methods change, from the old blind quarantine of the past, where the patient was permitted to eliminate infection within a demarcated area, into such expert nursing and treatment of the patient as destroys infection at its source while striving to produce a cure. Here the Health Officer of necessity passes into the territory of the physician, because to cure a case of infectious disease is also the most rational and economical way of destroying that source of infection. This in London is so completely recognised that there the public purse provides for poor and rich alike free treatment in such cases.

Health knowledge of the present day extends to every stage and every phase of the human career. It teaches that certain classes of persons should not become parents: advises mothers how to conduct themselves prior to the birth of their offspring; extends to the various modes of infant-feeding, showing which are harmful and which are not. It would make the school both a place for the children to be taught how to guard their own health, and where recreations and exercises would be designed to conduce to their highest physical development; would control the duration and conditions of all labour, and of cessation from labour, that as the rewards of labour accumulate the labourer may not decay.

It will be noticed that health work involves the employment of experts in other subjects than the details of human physiology. The engineer is as necessary to human health in the towns as is the instrument-maker to the surgeon, and the architect and builder to the asylum or hospital treatment of patients. But the ultimate object is human health; and the controlling mind in the adaptation of the work of experts in other professions to the needs of the Health Officer is essentially his own.

It may be objected that most of what has been said applies only to mankind when congregated together—to the dwellers in towns—and that therefore the country as a whole is not interested.

In part such an objection is true. For in the towns there is infinitely greater need to be on guard against these risks, which by congregation of humanity are manifoldly increased; where without collective action individual vitality would be lowered, and the risk of individual infection increased, and where the merchandise of flesh and blood increases the pressure upon those who are thrown on the labour market above any pressure which occurs to him who is living the simpler life of direct dependence upon the soil that is his own or his tribe's, or no man's.

But such an objection is valueless against any argument deprecating the importance of the welfare of the towns, because under present unavoidable conditions the gathering together of men in towns is a necessity—a necessity for education, for intercommunication, for manufactures, and for government. Even in Africa the country dwellers are a minority. And were the towns to suffer either in health alone, or in wealth through health, the farmer would be in fear of infection with the town's diseases, or would have cause to lament the impoverished market for his produce.

Unquestionably the services which sanitary knowledge has rendered to towns has been proportionately much greater than any benefits it has conferred upon the agricultural or pastoral classes, partly because the towns, having increased more rapidly in population and in corporate power, through the increased opportunity they afford for individual development and individual aggrandisement, have seen the necessity for health work, and have adapted it to their administrative machinery. One now hopes that country development may receive an appreciable impetus, and that there may be opened to reasonable calculation as great opportunities for individual prosperity upon the land as any that hitherto have been realised upon the mine or in the town. One can suggest directions of increased usefulness for the Health Department of the future in placing itself at the disposal of the countryman. He might look to it for expert assistance in choosing a site for his house, in its aspect, design, thermal insulation, and refuse disposal; also for free diagnosis of suspected cases of infectious diseases. One can also conceive a farmer finding it an invaluable relief to his mind when either the State or a corporate rural local authority segregates those from his neighbourhood who, through having contracted some dangerous or loathsome communicable complaint, are liable to convey it to others. Not the least important of the services which the State now renders to the farmer is his protection against smallpox by systematic vaccination, a matter which all experience proves cannot be trusted to the initiative of the individual citizen. But those suffering from tuberculosis and from that other disease which we do not always name, have never yet been isolated from rural areas. But beyond all these services one must recollect that there are vast stretches of fruitful lands in Africa awaiting their full development until such time as the fear of malaria, or of sleeping sickness, ceases to keep our more prosperous and competent farmers from migrating thither with their families—problems which in an undeveloped country like ours a State Department is alone competent to grapple with.

Let us face the obvious difficulty of inducing the citizen in health to take more than a theoretical interest in preventing disease —a difficulty which with us is increased by the large amount of preventive work at present proceeding, work which takes the sting from disease, and keeps it in the background.

Let us try and express the problems in terms of political economy —in terms of money.

It has recently been estimated by competent persons that, in England and Wales, the yearly loss in wages through inability to work due to tuberculosis is approximately sixty millions, or more than double the cost of the Imperial debt. It is less easy to estimate the corresponding loss in Africa, because the greater part of our population is in a more primitive condition of civilisation. The native is as yet not as continuously on the labour market as the white man, and when the native is not working for others his activities are not so great or of such a nature as appreciably to affect the trade profits of the country. Yet, were the native races preserved in health, their numbers might be so great as to force them upon the labour market to the exclusion of the Portuguese labour at present employed in the Transvaal mines. And I submit that if tuberculosis and other preventable diseases were scientifically restrained amongst them, the consequent increase in the wealth of our country might reasonably be estimated as equal to the present cost of the Union State debts. And, after all, our State, our *respublicae,* must be dealt with on a business basis, and that which is the common loss estimated and avoided.

Any such estimation would be simplified, or become unnecessary, should by any extraordinary circumstance all preventable disease become accumulated into one part of the year only; for then it would be so great, and so great a contrast to the preceding period of health that the business of the country would come to a standstill, and the populace would clamour for those remedial measures which their reasonable forethought failed to express any desire to obtain. The public as such are like the servant who draws sick-pay while ill. The man who realises most the misfortune of disease is he who has others and dear ones dependent upon his weekly wage, and who, when the wage ceases with illness, is thrown upon the charity of others or upon the result of his individual foresight in self-insurance against disease. What we advise is that the public should lessen the cost of individual insurance against the effect of disease by lessening disease. We advise that, in addition to the action the State takes in providing care and treatment for certain classes of its servants and citizens, in addition to providing care and treatment for railway servants, police, and paupers, it should also and more reasonably provide or foster all the known means whereby not they, but also the entire community, should be rendered more robust and guarded from the infection of disease.

What one means by a State "Health Department" is that the State should have the ultimate control of all these activities, organised under one public servant.

What one means by asking that there should be a Minister of "Public Health" is that a gentleman answerable to the citizens as a whole would find his time fully occupied in dealing with these affairs alone, and that these affairs are of sufficient consequence to the people to warrant such an appointment.

What one means by asking that the immediate subordinate of the Minister (the permanent public servant at the head of this Department) should possess qualifications in medical knowledge is only the generally recognised principle that for any particular work the most competent person should be chosen—one who, in addition to any legal administrative knowledge and capacity he might have, would also be versed in all the special expert knowledge of his subordinates, and therefore be competent to supervise their work, and whose subordinates could not require him to accept their views in the way a judge accepts expert evidence.

One may now ask: Why, granted that these services can be rendered by the State, should they not be left to be organised apart from the State, with a legal remedy for damage against any individual or authority through whose neglect injury to health might ensue?

The answer to this is that the citizen, from State administration (which he controls) should gain greater efficiency and save expense. The Health Officer is likely to accept orders from a Minister of State that he would not accept from any person of lesser consequence. And all persons will accept smaller pay for the same work when in State employ than when serving others who are either less solvent or less the fountain of honour.

The alternative of keeping health matters outside the administrative control of the State, and of making the relation of the health worker to the State that of a contractor rather than a servant, is one which probably would be found to favour the profession rather than the public, and therefore from the citizen's point of view, needs more careful consideration.

There are many instances of powers built up within the State which it has afterwards become a matter of political importance to restrain.

Those who framed the Union of the original British colonies in North America believed that under their laws all transport routes were public. Since then applied science has produced *de novo* "railways," and economic conditions have made them the essential inland transport routes of those States. Other economic conditions are forcing their fusion into one organisation, and already they have become a determining factor in the balance of political parties. On the other hand, African statesmen, having been called upon to face the railway development after such development had been fairly well advanced elsewhere, have kept it completely under State administrative control.

The framers of the mining laws, under which the right to mine continues to exist here, by having made the unit area of a mine about the size of a building lot, and by the conditions of grant, appear to have presumed that the actual white man working on the mine would always be its owner. They surely never anticipated the huge amalgamations of mining claims which now exist, producing a political power within the State confined to a few controlling persons, working in harmony and representing not the workers on the mines but the owners. It is too late now for the State to restore fixity of tenure of employment to those working on the mines, or to see that they again receive all the profits of their labours (beyond the cost of repayment of actual development expenses, and of actual present management).

History teaches us that in the days of the Italian renaissance the towns put out their defence to contract, with the result that their contractors became their masters.

What I understand our profession now to recommend is that we receive as much consideration from the State as does the legal profession; that certain of our members should be as much permanent State servants as are the judges under our constituting Act; that they should be reasonably paid—not necessarily as highly rewarded in money as are our leaders in private practice, even as at the Bar many a lawyer earns more than the judge before whom he pleads; that no lay official should have the right to himself question the soundness of their expert opinions, even as the Secretary to the Law Department cannot expostulate with a Judge of a Provincial Court for giving a judgment which is reversed on appeal.

Our profession would perhaps even desire that the State should refrain from attempting to artificially depress the market value of our services under any circumstance or in any place.

Supposing the medical profession is not to occupy such a position; supposing it is decided for it that it must always work outside the State; is there any reason why it should not attain to the dignity, the importance, and the relative wealth to which the officials of organised Christianity attained seven hundred years ago? Why should not all hospitals, asylums and sanitoriums be under its complete control? Why should it not have legal contracts by which every member would be as effectually bound to every other for the good of each as are railway shareholding and mining claims—contracts by which it would refuse to qualify others in the knowledge of the profession except it were known that they would join the profession for its strength, and not for its weakness, and so leave each member untrammelled by care for the necessaries of this life, and so free to devote his entire energy and attention to the needs of others? Might not even this be for the greater good of mankind? But it would not be democracy.

VI.—REMARKS ON THE ÆTIOLOGY AND PROPHYLAXIS OF MALTA FEVER IN SOUTH AFRICA.

By A. Garrow, M.D., Steytlerville.

At the outset I wish to say that what follows in this paper is nothing more than what the title says—that is to say, remarks. Towards the end of 1908 and during the first few months of 1909 there were abundant opportunities for making definite and precise observations on the above subjects, but various circumstances prevented me from making use of them. I have paid more attention to the clinical aspect of this disease that fitted in better with one's daily work.

In 1887 Colonel (now Sir David) Bruce, of the Royal Army Medical Corps, announced his discovery of the causative organism of Malta fever, and it has received the name of *Micrococcus melitensis.*

In 1906 the Mediterranean Fever Commission made the important discovery that the goat plays the most important part in the dissemination of the disease by discharging the micrococci in its milk and in its urine.

Through the kindness of Dr. J. A. Mitchell, the Secretary of this Section, I have had the opportunity of reading the first four parts of the Reports of the Commission.

The following are some of the results of, or conclusions drawn from, experiments made by the Malta Fever Commission, and described in these first four parts of their Reports:

(1) The micrococcus can be isolated from the urine of Malta fever patients, but they have not been isolated from the fæces, the expired air, or the sweat;

(2) The micrococci are present in the peripheral blood of most cases examined, but in such small numbers as to make it very difficult for the disease to be carried by biting insects;

(3) The inhalation or ingestion of infected dust, and the swallowing of infected food will give rise to the disease;

(4) In a Maltese soil allowed to dry naturally the micrococci were found to have retained their vitality for forty-three days, and in a damp soil for seventy-two days, and on dry khaki cotton, khaki serge, and blankets, for eighty days;

(5) Exposure to the sun for a few hours kills the micrococci;

(6) Micrococci were found to have been washed through a Chamberland filter, or Berkfield filter;

(7) Pasteurization at 68° C. for ten minutes destroys the micrococci present in infected goat's milk.

With regard to the conditions in Steytlerville district, the goat population of which, in 1907, was estimated as 266,343, I should like to make the following remarks. During the whole of 1908 and until March, 1909, the district of Steytlerville suffered from a severe drought. During that period disease was very prevalent amongst the goats, and there were many deaths. Farmers were almost unanimous in saying that the goats began to die before the scarcity of food and water was sufficient to be the only cause. I regret, now, that I did not get more blood specimens from these goats, so as to find out to what extent Malta fever was responsible for the disease and deaths amongst them. I sent only one specimen to the laboratory from Angora goats, and it reacted positively for Malta fever. But one examination is scarcely worth mentioning when we are dealing with a disease of which thousands of animals died. As I have already mentioned, several circumstances prevented me from taking up the work which an enquiry of this kind would have entailed.

During drought, fewer goat ewes conceive than during good seasons, and abortions are the rule amongst those that do. So that a farmer who gets six hundred or more kids in a good season may get less than a score at a kidding season during a severe drought. A ewe dries up almost immediately after an abortion. This extreme scarcity of kids, and consequently scarcity of goat's milk, was the general rule here in 1908, and yet during that time and until the drought broke in 1909 Malta fever became increasingly prevalent. That led me to think that during that period goat's milk was not the only, and, perhaps, even not the main, etiological factor in the spread of the disease, although, unfortunately, I did not make definite notes as to whether each case had or had not used goat's milk. I always made the enquiry, and the usual reply was: "We have not got enough milk for the coffee even." However, I did not go definitely and deeply enough into the question so as to make certain that each case who denied the use of goat's milk actually was absolutely without it. A very usual occurrence was that when I would inform a patient that he had Malta fever, he would say: "But how is it possible, when we have not had milk on the farm for months?"

Amongst other factors which I think must have played a very considerable part in the spread of the disease must be mentioned the swallowing of urine-infected kraal dust, and the fouling of the hands and inoculation through abrasions with infected urine in handling the urine-soaked fleeces of sick goats. Even if the disease which ravaged the goats at that time was some other than Malta fever, it is almost certain that many of them had the latter disease in addition to any other which may have been the immediate cause of death, and were discharging the *Micrococci melitensis* in their urine. During a drought there is much handling of sick goats, and the farm people spend much more of their time in the goat kraals than during good seasons; therefore the inhalation and ingestion of infected dust was very continuous, and for a prolonged period, which were the conditions which the Malta Fever Commission found necessary experimentally for the production of infection by these means.

Again, the aggregate number of goats slaughtered in a stock district for food purposes in the course of a year must be very great. On one extensive property in this district, for instance, about five hundred animals are slaughtered annually for food purposes to supply the households of the three owners and of about twelve herds. Boer goats are most commonly used for that purpose. Malta fever is an easily inoculable disease, so that inoculation of the farmer who kills and dresses the animal, if the former stabs or scratches himself with knife, saw, or bone spicule, or if he has, as is often the case, open sores or scratches on his hands or arms, and if the latter has micrococci in its blood, must be considered as one of the factors in the spread of the disease.

Milk from a healthy goat may become infected by the udder becoming fouled with infected urine in the kraal.

Children may become infected through the milk of infected mothers. I have sent to the Public Health Laboratory samples of human milk which gave positive Widal reaction.

The most dangerous part of the goat kraal is that which is protected from the germ-destroying sun by a galvanised roofing—the part known as the shed, or *schuur*.

The following means to check the spread of the disease would naturally suggest themselves:—

(*a*) Drinking water should be boiled;

(*b*) Milk should be strained and thoroughly boiled, and also that used for coffee and tea;

(*c*) Goat kraals should be built as far away from the house as possible, and on the prevailing leeward side of them. Sheds should be fenced off from the kraals, and only used when the weather conditions demand it. Goats which are visibly sick should be isolated from the apparently healthy:

(*d*) In slaughtering and dressing goats, care should be taken not to wound the hands or arms with the knife, saw, or rough ends of bone, and the hands and arms should be well cleansed afterwards. In making *post mortem* examinations on dead goats, which is very frequently done, even greater care should be exercised;

(*e*) After attending to sick goats, the hands should be thoroughly cleansed;

(*f*) Infected mothers should not suckle their children;

(*g*) Those who nurse cases of Malta fever should be instructed to handle carefully and disinfect the fæces, and more particularly the urine.

VII.—EPIDEMIC MALARIA.

By H. A. Spencer, M.R.C.S., L.R.C.P. (Lond.), Middelburg, Transvaal.

Ten years ago it was tritely observed by Sir Patrick Manson that every year it was becoming more apparent that the student of medicine must be a naturalist before he can hope to become a scientific epidemiologist or pathologist or a capable practitioner. The truth of that observation has since become abundantly manifest, and to no disease is it more applicable than to malaria. It is principally the study of the conditions favourable to the development of the mosquito which to-day occupies our attention, for the extermination of malaria means the creation of conditions unfavourable to its production.

In every work on malaria, Africa is, in a general way, credited with being one of the homes of the disease. But we are yearly becoming aware that, in spite of its greater salubrity compared with the central and northern parts of the continent, Southern Africa is pretty generally affected too, though the lower, more sheltered and better watered parts are doubtless more severely affected than the higher and drier situations; we know, however, that malaria is unaffected by altitude *per se,* that wherever mosquitoes can breed and man harbouring the parasite can go, there the disease can become prevalent sporadically, or even in epidemics.

In enquiring for the commonest means of conveyance of the disease throughout the country, we must first point to the wandering habits of the native, from Central and Eastern Africa to the cities and towns of the Transvaal, and from the low-lying country to the locations and towns.

Of the infection by, and tolerance to, the parasite of malaria by the natives races, literature supplies the following:—

(1) "The presence of parasites in the blood of the fœtus of malarial mothers has been repeatedly demonstrated."

(2) "Babies are born with malaria, exhibiting at birth enlarged spleens, cachexia and œdema of the feet, of which they often die during the early months of life."

(3) In the first years of life particularly they predispose to infection, but "a relative tolerance is, however, soon acquired, and it is often remarked that these children, with enormous spleens and a rich stock of malarial parasites in their blood, run about fever free and apparently in rude health."

Some of these conditions are within my own experience, and I am convinced that the native of South Africa should everywhere be suspected of harbouring the parasites of malaria, and therefore of being a nucleus of infection.

The railways may also convey the communicating mosquito from Delagoa Bay to Cape Town, or from the low veldt of the Transvaal to parts hitherto salubrious in Cape Colony. Ships convey infected individuals and infected mosquitoes all over the world. Manson attributed the great epidemic of malaria in Mauritius in the early sixties to this cause.

Two types of malaria, differing widely clinically, are generally described—

(1) Benign, due to infection by one (or both) of two parasites, giving rise to symptoms which, while they are of a comparatively mild or benign type, have certain definite characters. This is the commonest type met with, and referred to popularly as "ordinary malaria." It is accompanied by rigors, ague or "koud-koorts" and pyrexia, recurring every forty-eight or seventy-two hours when uncomplicated by a double or mixed infection. In general terms the characteristics of this type are expressed in its title.

(2) Malignant, due to infection by one of three forms of parasite, gives rise to much more severe, even serious, symptoms of a more lasting and profound nature, and more resistent to ordinary treatment. In this form rigors are often not marked, but relapses are liable to occur for long periods, accompanied by cachexia, bone pains from infection of their medulla, and leaving those affected ruined in health it may be for years. It is this malignant type of malaria, in which deaths frequently occur in those untreated—the "fatal form of malaria" referred to in the older records—which has occurred in epidemic form in various parts of the world, and which has of late years also visited South Africa.

The Island of Hong-Kong was previously "healthy enough," but shortly after its occupation by the British extensive excavations were undertaken for the construction of the city of Victoria, for the building of barracks and houses, and for the laying out of roads. These works were accompanied by "a fatal form of remittent fever" of which soldiers and civilians died by the hundred.

During the civil wars in the Southern States of America the country was uncultivated for years; at the termination of the war, "*when cultivation was once more renewed*," epidemics of malaria in a malignant form hitherto unknown made their appearance.

In every record of epidemics of malaria with which I am acquainted there have been but two explanations of them given. Most commonly they have accompanied "disturbances of the soil" on an exceptionally large scale, and have attacked first those so engaged; the other explanation given is that they occurred "upon the subsidence of floods." And this history is but repeated, whatever the explanation may be, in all the epidemics in this country which I have had the opportunity of investigating.

Manson's explanation of the former cause of epidemics is that "soil disturbance implies the formation of holes, holes imply puddles, and puddles mosquitoes." That the epidemics imply mosquitoes must now-a-days be granted, but the implication of the formation of holes, carrying sufficient water for the time necessary to enable the larvæ to develop, I have not as yet been able to confirm, nor by discovering the source of the mosquitoes to substitute any other explanation. In some cases the source of the mosquito was patent in the irrigation furrow or other available water, but such instances were comparatively rare, and confined to the affected Europeans. As a rule there was, on careful investigation, no evident source of the mosquitoes amongst the scattered thousands of affected natives, unless it be possible for them to develop in the earth of their extensive tracts of tilled lands when kept sufficiently wet for the time necessary for their development. This tillage is sufficient to convert a practically impervious into a porous soil which would retain rain-water for a sufficiently long period for the development of mosquitoes; the lands are invariably ploughed across the slope of the ground to still further conserve the rainfall, and no artificial irrigation was anywhere in use amongst the natives to supplement the rainfall, nor are draining channels, which might encourage small collections of water, ever found in these fields. Yet in every epidemic the association of infection on a very large scale with these cultivated lands is unequivocal. It commences there; large numbers of natives continue to be there infected until the lands are deserted on account of this association with the infection. Doubtless experiment will sooner or later set this matter at rest, determine whether it is possible for the larvæ of mosquitoes to develop to maturity in loose soil kept wet or damp for a sufficiently long period, and establish the true connection between malarial infection of epidemic dimensions and this "soil disturbance." From our experience of excavations, road-making and such-like soil disturbances, the association of holes and puddles in the wet season is more easily understood, but if, added to this source, that of the possibility of mosquitoes breeding in the porous damp earth were proved, the rapid spread of these epidemics could better be understood. The second condition which has been associated with epidemic malaria is their occurrence "upon the subsidence of floods." Here Manson's explanation is, in my experience, obviously the one applicable, and here, as elsewhere, is confirmed by the time of year at which these epidemics occur. He says: "*Short of general overflow*, the higher the subsoil water the greater the chance of a given locality supplying places for mosquitoes, and therefore of proving malarious."

During the whole of our summer the rainfall is sufficient to perpetually flood the soil, flushing pools, drowning and washing away developing mosquito larvæ; it is upon the subsidence of the rains that in the Transvaal the epidemics of malaria arise. This no doubt accounts for the time of occurrence, the absence of epidemics some years depending upon the absence or curtailment of favourable meteorological conditions, principally of temperature, which encourage the rapid development of myriads of mosquitoes. Thus, what is necessary to the development of the larvæ is that the mean temperature should not fall below the critical point of 60° Fahrenheit, this ordinary period being accelerated by sustained hot weather—probably a mean temperature of about 70°. When the temperature falls below the critical point, mosquitoes become inactive, and take shelter in some darkened nook. The effects of temperature upon the parasite developing within the mosquito are also worth noting. Thus, upon the temperature falling below the critical point, its development ceases, and when sustained it perishes; on the other hand, during sustained hot weather—say a mean temperature of 70°—this development is accelerated in a more marked manner even than in the case of the larvæ.

A mean temperature of over 60°, therefore, is necessary for the development of mosquito larvæ and the parasite within the mosquito, sustained hot weather accelerating that development in both.

In the Transvaal these favourable conditions obtain all summer into autumn, but the rainfall is unfavourable to mosquitoes until a varying time during the æstivo-autumnal period; if at this time temperature conditions are also favourable, mosquitoes are hatched out in sufficient numbers to create an epidemic of malaria. When these conditions cease to be favourable—if quickly following the cessation of heavy rains or after a varying interval—epidemics do not occur, or come to an abrupt termination when they do. The following facts connected with mosquitoes are, I think, of sufficient importance to reproduce in this place.

The process of development from egg to imago ordinarily takes about a month, but under the favourable temperature already referred to this may be slightly accelerated. It is only the female which sucks blood, the male living upon the juices of plants.

Young females produce eggs within a week or ten days from their emergence from the pupal state, and each one may lay eggs many times in a season, many hundreds each time; each pair can therefore, under favourable conditions, produce several millions of their kind in the course of one summer.

The parasite in the mosquito, from the time that it is taken with the blood into the stomach until the sporozoites appear in the mosquito's salivary glands, takes about eight days to develop; it is then ready for transmission into the human subject. As already mentioned, this "mosquito cycle" may be considerably shortened by sustained hot weather. To "cultivation" of the soil, "disturbances" of it, the "breaking up of previously fallow soil"—to all of these have been ascribed epidemics of malaria in some part of the world; whilst to "cultivation of the soil," *e.g.*, by raising crops upon it, tree-planting, etc., is attributed also the antagonistic condition of extermination of malaria. Whatever kind of cultivation thoroughly drains the soil will, of course, be anti-malarial, but that which renders the soil more porous and encourages the stagnation of water in it will, on the other hand, encourage malaria. The question hinges upon drainage of the surface soil.

During the summer months uncultivated ground in malarious countries is usually covered closely with vegetation, the roots and rootlets of which fill the surface soil and quickly absorb percolating moisture; this surface soil is probably sun-baked, hard, and but little pervious, so that most of the rain-water runs off it, the little percolating being absorbed by the roots of the vegetation. This condition is not conducive to the breeding of mosquitoes.

Likewise, when trees are planted, their roots and rootlets, increasing in size and spreading out in every direction year by year, drain the soil more and more effectively of all excess of moisture. In particular, the cultivation of trees is credited with ultimately and permanently banishing malaria from many tracts of country in previously malarious parts of the world.

But supposing that the fallow ground referred to is, for the purpose of cultivation, extensively broken up? The natural absorbents of excess moisture are exterminated, and the surface soil is converted from an impervious into a porous one; furthermore, the ground is cross-ploughed to conserve more moisture. In this soil, saturated during most of the summer, cereals are grown for a few months in each year whose roots occupy but a small part of it, and are very far from draining it of the moisture it retains and which is unable to run off. Surely *this* cultivation will never sufficiently drain a soil to prevent mosquitoes developing in it? Rather will it act as a nursery for them. Again, is it *puddles* that this kind of cultivation implies, or is it not possible that damp, if not wet, earth is sufficient for the larvæ, protected as they would be from their enemies of the pools, spruits and tanks?

Of epidemics of malaria in this district (Middelburg, Transvaal) I was surprised to learn that previous to 1896 the disease was unknown here, at any rate, in epidemic form. In that year the most severe epidemic yet experienced swept through the teeming native population of the low part of this district. Although malaria of any description, previous to this year, is generally denied by both the natives and the few Europeans living in these parts, I consider it possible that the slighter feverish attacks did occur, and were tolerated and disregarded. However this may be, this first epidemic was entirely novel to these people, who had never before experienced any disease of this nature; it was therefore given the name of "the Mashona sickness," or merely "the Mashona," to indicate that it was imported, and not previously known in those parts, and the subsequent epidemics with which I am acquainted were called by the same name and recognised as identical with this first one. Its origin was clearly associated with disturbance of the soil in connection with railway construction through this district and contiguous parts of the low country to the eastward. Thousands of natives were recruited from the neighbourhood for labour on this line, hundreds of whom fell sick in the camps and died there; many, however, as soon as they were able, crept to their homes only to die there of recurrences and to convey the sickness to all the large kraals and stads. My first impression was that this epidemic was one of enteric fever, for it occurred some years before my arrival in the Transvaal; it was only after repeated and strict enquiry, "weighing of evidence," and especially in the light of conversations I had with a particularly intelligent European missionary who had lived in the heart of this country for many years, that I recognised it beyond any doubt as the "bilious remittent fever" of which epidemic malaria always consists. This epidemic "was still raging in the month of the Jameson Raid," I am told, which would bring it into the æstivo-autumnal period of that year. Hundreds of natives died in the railway camps and in the stads and kraals, so that there was no labour for the fields during the following summers. The railway construction passed on to the East, and the epidemic concluded abruptly with the first frosts; but partly owing to the heavy mortality creating a great shortage of labour, and partly owing to their possession of relative wealth as a result of their labours, practically no cultivation was practised in these parts from that time until "some years after the war."

In 1905, however, cultivation in these parts commenced to assume proportions never before experienced. Miles of country were being broken up for cultivation in every part, by both Europeans and natives, in increasing extent year by year. The crops planted generally were mealies, but in addition to these the natives planted Kafir corn or millet.

In 1906, just as in America after the Civil wars, "when cultivation was once more renewed," suitable conditions as regards rainfall and temperature occurring in January, the second epidemic known in this district set in. Investigation, during which I saw several hundreds of cases, showed it to be what is usually described as "bilious remittent fever."

Europeans affected were saffron-tinted in various degrees, natives showing this only in their sclerotics; the pyrexia, seldom attended by rigors, but with the usual headache, body pains, etc., remitted (usually in the mornings) and was seldom as high as 103°; abdominal symptoms were invariably present, consisting of epigastric pain, vomiting, pain and tenderness with some enlargement of both liver and spleen; constipation was the rule, though loose motions sometimes occurred later, with nothing characteristic about the stools. In most cases one was much struck by an eruption of purpuric spots varying to petechiæ, by nose bleeding, and by more or less bronchial catarrh, which was often described as a precursor to the onset of illness. This in a general way describes the symptoms of the majority, who until my arrival had received no sort of treatment. It may be that to this fact was owing the quite common tendency to heart-failure, whilst in quite a number the symptoms recorded as associated with malignant subtertian malaria were exemplified; it was amongst these that the deaths occurred. Thus the type of infection described as "typho-malaria," "adynamic" and "pernicious" remittent fevers were all before me amongst those affected in this epidemic. These serious conditions are described as due to the stasis in the capillaries caused by adherence of the parasite-laden blood corpuscles to the walls of the vessels; they were generally preent, in varying degree, in those untreated, after two or three weeks' illness.

This epidemic appeared to be abating a month after its commencement, but it dragged on, with diminished incidence, for another month, until with a fall of the temperature at the end of March and the advent of frost early in April it came to an abrupt conclusion. I estimated the incidence to be about fifty per cent. of the population, and the death-rate about ten per cent. of those affected, but in the absence of all observance of notification of deaths amongst the natives, it is only possible to roughly compute the figures.

I was much impressed during my investigations at the universal expression of the fact that for some time only those living near or working in "the lands" were affected, and came back to their kraals and stads in large numbers to apparently convey it to their children and women. There was a constant mortality amongst pregnant women who had received no treatment. Quinine was freely distributed, and undoubtedly saved many lives.

Blood smears were taken from a number of cases in various stages of infection, and were submitted to the Government Bacteriologist for report; they showed that only young forms of the parasite and ring forms were present, no crescents or pigmented forms being seen in any of them. Bearing in mind the difficulty due to the absence from the cutaneous circulation of the forms characteristic of the malignant type of infection, I was still convinced that those seen during the epidemic were not only infected with the benign, but with the malignant parasite superadded. This conviction has been verified lately by the finding of malignant forms of pigmented parasite in a patient exemplifying all the characteristics seen in these epidemics, who died within forty-eight hours after coming under treatment, and from whose spleen, *post mortem,* I secured blood smears.

The rainfall of the following summer was far heavier than usual, being some ten inches over the average, so that up to the middle of February, 1907, the ground and subsoil water would be constantly and at short intervals in flood; but from mid February until the end of March conditions were highly favourable to the breeding out of mosquitoes—the rainfall being sufficient to maintain the ground always wet, and the mean temperature keeping about 70°, and towards the end of March sporadic cases of bilious remittent fever were reported all over the low country. By this time, however, the mean temperature had commenced to fall rapidly and heavy rains re-commenced, which averted what appeared to be a recurrence of the preceding year's epidemic.

During the season 1907—1908 though cultivation was increasing rapidly, the meteorological conditions were quite unfavourable in the æstivo-autumnal period for the production of mosquitoes, and not even the ordinary intermittent "ague" was reported from the low country The epidemic of 1909, however, was the severest in my experience, and was everywhere described as almost as severe as that in 1896.

The rainfall of the summer was throughout heavy, and was prolonged until March, 1900. Under these circumstances, and from the fact that at the end of March not a single case of even the ordinary intermittent fever was reported throughout the low veld, it was confidently anticipated that there would be no malaria that season. But the rainfall rapidly declined, favourable conditions of humidity and high mean temperature were maintained during April and May, and once more bilious remittent fever in epidemic incidence swept over the district and lasted until early June. At this time the amount of cultivation in view was, to anyone accustomed to the ordinary fields, simply astonishing! In parts of the country it was not possible to see uncultivated veld anywhere from where one stood. The concurrence of a heavy incidence of the fever, too, where cultivation was most extensive, was very striking.

The epidemic was not confined to the low country, but occurred also on the higher plateaux, though the mortality here was not so great. Thus epidemic malaria occurred at Pan at an altitude above sea-level of 5,390 feet, Wonderfontein at 5,904 feet, and Belfast at 6,295 feet, as severely and with an incidence quite equal to that of the low veld; at all these places the tracts of country under cultivation ran into many miles, and at some of the kraals adjoining these large areas scarcely a man, woman or child but was down with the fever.

Throughout the low veld the incidence was similar, whole kraals, even stads, being affected to the extent that scarcely an individual stood upon his legs. The symptoms were precisely those already described in the epidemic of 1906, but the incidence was greater, and the mortality higher; as in that year, too, the first attacked were those working on, or living in, the immediate vicinity of tilled ground.

The incidence during this epidemic I compute, from my own observation, and from returns supplied by the various police posts throughout the low veld, to have been seventy-five per cent. of a population estimated to be 32,000, and the mortality twelve per cent. of those affected.

Blood smears made during this epidemic and submitted to the same authority gave similar results, young and ring forms of the parasite only being found.

Whilst there is every hope of considerably reducing these malarial epidemics amongst Europeans by introducing prophylactic measures, I very much fear that but little can be achieved in this direction amongst the natives. Their kraals and stads, however, might with great advantage be removed to higher and sloping ground, and they may be warned to profit by their experience and only visit their cultivated lands during broad daylight. To Europeans, however, I should recommend the following:—

(1) Irrigating furrows must not be allowed to exist within half a mile of dwellings, and should be carefully avoided after sunset; they should be made as nearly V-shaped as possible —instead of flat-bottomed—the more readily to admit of their being flushed with but little water, and to avoid the formation of puddles.

(2) Something might be done to drain away excessive rain-water from cultivated ground, without interfering with a sufficient water supply to the growing crops. Introduction of the more modern methods of soil treatment by "putting a better surface" upon it might assist in abolishing collections of water.

(3) Considering the possibility of every native being the host of malarial parasites, their dwellings should always be well removed from those of Europeans; especially is this the case with native employees in houses.

(4) Methods for the exclusion of mosquitoes from houses by wire-gauze coverings to windows and doors need only a passing reference.

(5) Prophylaxis by means of small doses of quinine, so useful in the intermittents, are not of such use in malignant infections, and cannot be relied upon.

With regard to treatment, it may be said of the affection met with in these epidemics that quinine, though it acts more slowly, does act efficaciously. A few doses, however, are useless in the prevention of the recurrences so characteristic of this type; its use requires regularity and continuance of treatment.

Of other suggested antidotes to this disease—*e.g.*, methylene blue and cassia—I have as yet had no experience.

In addition to quinine, and as an adjunct, I find digitalis and strychnine of very great assistance in the treatment of asthenic cases, many lives amongst those coming late under treatment having been saved, I consider, by their use. Meteorological observations of temperature are unfortunately not available for the low country; and records of mean temperature through the æstivo-autumnal period would be of special interest.

VIII.—MALARIA: SOME ASPECTS OF THE RECENT EPIDEMIC IN THE ORANGE RIVER VALLEY, FROM THE POINT OF VIEW OF THE GENERAL PRACTITIONER.

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When the Honorary Secretary of this Section invited me to submit a paper dealing with the subject of malaria to the Congress, I felt that it would be impossible for me to deal satisfactorily with the scientific aspect of the recent epidemic owing, in part, to the fact that in the storm and stress of the enormous amount of purely administrative work this aspect was forced into the background. Yet the fact of so sudden, severe and extensive an outbreak occurring in a region apparently previously free, or almost so, from malaria was so remarkable that it seems to me that some account of the epidemic from the point of view of the general practitioner, might not be uninteresting to the members attending Congress, who might similarly be faced with such an epidemic in such a dramatically sudden manner.

General Considerations.—Before going into a description of the 1909 epidemic it will be necessary for me, if I am to advance a theory as to the cause of the outbreak in these parts, to remind my audience that the Orange River drains the vast extent of country lying west of the Drakenbergen, Wittebergen and other ranges which form the watershed of this part of South Africa. In brief, by means of its tributaries, the Vaal and Riet Rivers, and by means of innumerable small branches, the Orange River drains part of the Cape Province, the Orange Free State, Basutoland and the Transvaal besides Griqualand West and Bechuanaland. Its banks are densely wooded almost to the mouth. In the last three or four hundred miles of its course it passes through the barren waste of sand, stones and kopjes comprising the Districts of Gordonia and German Namaqualand on the north, and Kenhardt and Great Bushmanland on the south. It is with this portion of the river, and with the districts of Gordonia and Kenhardt that this paper is chiefly concerned.

The district of Gordonia extends, roughly, for one hundred miles along the north bank of the Orange River, which divides it from the district of Kenhardt. It is bounded on the east by Griqualand West, on the west by German South-West Africa, and loses itself two hundred miles to the northward of the river in the Kalahari Desert. It comprises 18,499 square miles of country, and has a population of 8,811 (1,712 whites and 7,099 coloured), according to the census of 1904. Two-thirds of the population reside in the riverside villages of Upington and Keimoes, and on the farms along the river margin. Almost the whole of this section of the population of the district is connected either as owners of farms or as labourers with agricultural pursuits. They live in huts or small brick houses built either in the islands in the river or in close proximity to the river bank. These one- or two-roomed dwellings are all overcrowded (as many as eight sleeping in one room), and, in the majority of cases, the floor is used as a sleeping-place. The coloured labourer lives in a state of poverty, which was more than ordinarily in evidence in 1909, owing to circumstances which will be described later.

The land cultivated by these people is irrigated from water furrows, with edges overgrown with weeds, and slowly-running streams. The water reaches the lands in small irrigation channels with lateral branches which are often choked with weeds. Both the furrows and the irrigation channels form ideal spots for mosquito breeding on account of their tortuous course, their weedy condition, and the pools which they frequently form.

The Rainfall of this district for 1908 was 1.99 inches, while in 1909 it reached the abnormally high figure of 12 inches. As in other years, the rainfall was highest in February, March, April and May. The rain filled all the vleis and artificial dams in the district; it also filled all the natural hollows and depressions, so that pools and puddles were abundant, the waters in which were, on account of the saturated state of the ground and waterlogging, not able to filter away as readily as is the case in other years with a smaller rainfall.

The Climate, which is usually cold and bracing from June to the end of August, was, in 1909, extremely mild and humid, with no frost, a fact which perhaps explains the number of mosquitoes which persisted through the cold season.

The River, owing to the increased, or abnormal, rainfall in the Transvaal and other parts drained, as I have shown, by the Orange River, rose in these parts twenty feet, and inundated all the low-lying irrigable land along the river's edge, and submerged many of the islands in the river bed, filling up pools and backwaters which remain empty, as a rule, all the year round. On the subsidence of the water many stagnant pools were left on the margin of the stream proper and in the holes washed out of the lands by the flood, and miles of silt and decaying vegetable matter were left deposited on the lands exposed to the sun's rays, giving off an unbearable stench.

This inundation, by removing their work, caused the increased poverty among the coloured people spoken of above, which poverty brought them speedily to a physical state in which they fell easy victims to the epidemic. Later on, when they were able to begin ploughing, levelling off the lands, and sowing, they had to work with the disadvantage of impaired nutrition on lands saturated with foul-smelling silt—lands probably, as I shall show, swarming with mosquitoes, to the effects of whose bites they easily succumbed.

Previous Epidemics of Malaria and their Relation to Rainfall and Floods.

I am told by old residents that similar floods and similar abnormal rainfalls occurred in the early months of the years 1881 and 1898, and that in 1881 the flood was followed by a very severe epidemic all along the river of "intermittent fever." The flood in 1898 was not followed by any epidemic. In April and June, 1900, a localised epidemic of benign tertian occurred, mostly on the farms on the river's edge eighteen miles east of Upington. This followed an abnormal rainfall, for the months of March and April, but never became general, and was soon stamped out. There was no flood in this year. Since the latter date—a period of nine years—there has been no further epidemic of malaria in this or the adjoining district of Kenhardt, although I have seen undoubted cases of malaria during the period named, either recurrences of the small 1900 epidemic or recurrences in persons travelling through the district who had become infected in other parts of South Africa or in other countries.

To sum up the physical conditions which existed in these districts before the epidemic under consideration, we find four factors concerned in the bringing about of such an outbreak:—

(1) A general lowering of the resisting powers of the bulk of the inhabitants, due to their poverty-stricken condition.

(2) Physical features of such a kind as determined waterlogging, and the collection of large sheets of water on the surface.

(3) Rainfall of an abnormal amount.

(4) A portion of the population, at least, already infected by previous epidemics, whose blood contained malarial parasites in an asexual stage, which only required to be kindled into a flame to produce a virulent epidemic.

(Daniels, quoting Anton Breinl, states that the latter has found asexual stages in the blood of persons seven years after infection.)

Bearing in mind these factors, we may proceed to consider the epidemic itself, its cause, the means adopted to check its spread, and its complications, near and remote.

The Epidemic.

This began with great suddenness practically simultaneously all over that portion of the districts of Gordonia, Kenhardt and Bechuanaland which constitute the Orange River Valley, and raged with great intensity from mid-April until the end of August.

From notes taken at the time, I find that the first cases which came under my observation were of the regularly intermittent type of fever (similar in all respects to those seen in the 1900 epidemic), whereas those treated from May onwards were in the great majority of cases of the irregular, intermittent or æstivo-autumnal type.

I. *The Regularly Intermittent Type.*

Tertian, quartan and double tertian types were seen with their regularly occurring paroxysms, accompanied by the classical symptoms which may be divided into—

Prodromata, of a few hours' to several days' duration;

The cold stage, when the rigor sets in;

The hot stage, accompanied by vomiting, headache and hyperpyrexia;

The sweating Stage, with a rapid decline of the fever, and return of the patient to the normal, with the return of the paroxysm in twenty-four, forty-eight, or seventy-two hours.

It is impossible for me to say what percentage of cases were tertian, what quartan, or what quotidian. From the clinical point of view they were all "cases of malaria" which required quinine treatment, to which drug, I may add in passing, they proved much more amenable than the malignant cases to be described later. One point is noticeable, and that is that children suffered mostly from the quotidian type. Many people suffering from these types got well without any treatment whatever, and many cases recovered with one or two ten-grain doses of quinine. I was, moreover, amazed to find that some, at all events, of these incompletely treated cases got no recurrences. These facts led to considerable doubt among the laity, who refused to be persuaded either that treatment was necessary or that the cases of the remittent type seen later, which were not so amenable to quinine, were really cases of malaria at all. Hence the later portion of the epidemic was labelled even by some trained observers "typhoid," "cerebral influenza," or "cholera," according to the most prominent symptoms of the cases. However, the examination of films, taken from quinine-free cases at different spots, by Dr. Thornton, of the Health Department, disclosed the *Plasmodium præcox*, and clinched a diagnosis which had been made on clinical observations alone.

II. *The Irregular, Remittent or Æstivo-Autumnal Type.*

The great bulk of the cases in the later part of the epidemic were of this type, and the clinical forms met with were bewilderingly varied.

These cases are ushered in with the same prodromata as the tertian fever, only the temperature rises more gradually. No periodic paroxysms occur, but some chilliness is complained of. When the temperature reaches 103° or 104° F. it becomes continuous, the tongue is furred, but very unlike the tongue in enteric, the breath is very foul, the bowels are generally constipated, there is often slight bronchitis, the spleen is enlarged and painful, and there may be slight jaundice. Persons suffering from this type of fever suffered great prostration and sometimes became delirious, but I think this was the result in many cases of previous insufficiency of food, as well as of the want of proper attention during the attack. The illness usually lasted from ten days to three weeks, the temperature falling gradually to normal.

Among the numerous variations of these symptoms were typical ague paroxyms (with cold, hot and sweating stages as in the benign tertian cases), after which the temperature would remain normal, and the patient soon convalesced; or bilious vomiting would set in, with deep jaundice—bilious remittent; or diarrhœa of a dysenteric character would replace the constipation usually associated with this type. Intercostal neuralgia of an acute type was often complained of, and the heightened breathing rate and anxious look led one to think of pleurisy. It is remarkable that neither this nor the previous type was accompanied by pneumonia, despite the season of the year and the small protection afforded by the huts occupied by the poorer sufferers. Dr. Theron, of Kakamas, has also remarked on the rarity of pneumonia during the epidemic. I frequently saw cases who had had a typical attack of benign tertian early in the epidemic who were suffering from such an attack of remittent as has just been described.

In my first cases of this type, being doubtful of the diagnosis, I probably gave too small doses of quinine, and to this I attribute the paroxysms which, as I have said, sometimes suddenly varied the continuous fever.

When the epidemic was at its height, I can say that quite seventy-five per cent. of the whole population of the district was down with malaria. Practically every farm in the back-country was attacked. Very often one would hear of whole households being ill at the same time, with nobody well enough to come into the village for medicine. Whole families of coloured people were wiped out, and, indeed, when one thinks of the poverty and distress that prevailed at the time, the fact that very often, although there was food in the house at the time, there was no one well enough to cook it, one wonders that more deaths did not occur.

Although it is impossible to judge of the amount of suffering that prevailed, when I state that in certain places work was at a standstill as a result of scarcity of labour, the economic loss to the district can be imagined.

The death rate of this district rose from 205 for all diseases in 1908 to 458 for all diseases in 1909; of these latter, 287 were returned as from "malaria."

Of course, when the pandemic nature of this outbreak is remembered, and when to the lay mind every ache and pain to which the flesh is heir was "malarial," and we medical practitioners even were, from very force of overwhelming circumstances, imperceptibly drawn to a similar state of mind, it is natural to suppose that many cases of death ascribed to "malaria" and returned as such by the lay Registrars were perhaps due to other causes, yet there cannot be much doubt but that this return of 287 deaths from malaria is not very far from the truth.

The Causes of the Epidemic.—Of the three requisites to malaria—water, man and mosquitoes—the latter only is left for consideration.

The culex abounds every year during the summer months all down the river valley, breeding in the riverside pools and "pans," and disappearing with the advent of the winter frosts. In the autumn of 1909, however, the culices, instead of disappearing as usual, began to increase until the air was thick and our nights were a burden. They were accompanied by two species of anopheles, one of which I have every reason to believe was *Pyretophorus costalis.**

Be the species what it may, there can be no doubt that anopheles abounded from March to November of 1909, this fact being attested by even the most ignorant observer.

I have two theories to advance to account for the presence of the anopheles:—

(1) When the Orange River came down in flood, it is feasible to suppose that it swept clean the breeding-pools in those parts of South Africa where malaria is endemic and anopheles abound, and deposited the anopheline ova on the ground at the margin of the stream, which, in its subsidence, left pools and backwaters in which the anopheles hatched out. These anopheles became infected by feeding on the blood of persons carrying the malarial parasites (perhaps in an asexual stage—the latent relics of a previous epidemic), or on the blood of trekkers fresh from malarial attacks in some other

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* Since writing this paper anopheles have again appeared at Upington after an absence of eleven months, and Dr. Peringuey. of the S. A. Museam, has kindly verified the genus of specimens sent to him as *Pyretophorus costalis.*

part of South Africa, and proceeded to inoculate the poverty-stricken workers on the lands. The contra argument that the floods of 1898 were not followed by an epidemic of malaria might be met by the supposition that the floods higher up the river were not sufficiently high to reach the breeding-pools, or that the flood water came as a result of rainfall in a part drained by the river where malaria and anopheles are non-existent.

The further argument that the epidemic of 1900 was not connected with a rise of the river might be explained by the circumstance that the late war was in progress at that time, and that the troops may have brought the anopheles and the parasites with them into the portion of the district affected.

To complete this theory, it could easily be imagined that infected mosquitoes were conveyed to the back-country from the river in the numerous wagons which come in from these parts to load grain and fruit at this season of the year, the mosquitoes using the wagons as hiding-places during the day time. Once in the back-country their migration from farm to farm, and from pool to pool would be easy.

(2) The prevaling winds during the months January to March, 1909, were from the north-north-east, and it is reasonable to suppose that these winds may have blown the anopheles, already infected in other parts of South Africa, down the river valley. It is known that, although the anopheles originally fly only about a mile from their breeding-places, with persistent winds from one quarter they may be blown for considerable distances when protected by trees. The mosquitoes thus blown found breeding-places in abundance, and every requisite for an epidemic.

This theory would account more readily than the previous one for the "general nature" and "sudden onset" of the epidemic noted above, also in a measure for the simultaneous attack of spots hundreds of miles apart.

Personally, I am not in a position to say *which* theory is correct. Either or both would explain the facts.

THE MEANS WHICH MAY BE ADOPTED TO CHECK FUTURE EPIDEMICS, INCLUDING THE MEANS ADOPTED IN THE RECENT EPIDEMIC.

(1) To raise the resisting power of the poorer part of the population by better food, clothing and housing, and by definite instruction of the young in the mosquito-malarial theory, as it is in the coming generations that our hope of sanitary progress lies.

(2) Protection of man against mosquito bites by mosquito nets and gauze screens.

(3) Mosquito extirpation.

(4) Quinine prophylaxis.

With regard to Nos. 1 and 2, the education of all classes might be compassed by the broadcast distribution of literature dealing with the question of sanitation; popular lectures might be given by competent persons, and school children instructed by their masters. An attempt was made in this direction during the recent epidemic, and mosquito nets were adopted in the houses of the better classes with success.

With regard to No. 3, during the recent epidemic it was rightly considered that any attempt at mosquito extirpation on a large scale at the hands of persons unskilled in such schemes would have been worse than useless, even had there been labour to carry out such attempts or had funds been forthcoming. Any attempt to carry out any large protective measures, *e.g.*, against river encroachments, would be impossible unless State-aided and, unless malaria becomes endemic in these districts, I do not think that any future attempts at stamping out the disease will be developed along these lines, owing to the length of river margin to be dealt with and the enormous expenditure of labour and money that such works would entail.

With regard to the destruction of larvæ, I am of opinion that the introduction of the ova of fish which are known to feed voraciously on larvæ, by the municipal authorities, and the introduction of certain water-plants, which are known to choke the larvæ, into breeding-pools along the river margin, as well as, perhaps, into the stream itself, would have good results. The fish known as "millions," which have been used so successfully in anti-larval crusades, would, if they could be got to breed in our river, be most useful in this connection.

With regard to No. 4, quinine prophylaxis on a large scale was adopted during the recent epidemic, and proved most successful. The quinine, supplied by the Cape Government and by local philanthrophy, was issued through the District Surgeon in mixture or in the form of tabloids by a committee consisting of the ministers of the various religious bodies, presided over by the Acting Resident Magistrate, who took an energetic interest in all measures for the alleviation of the distress among the poor in the district.

A house-to-house visitation was instituted, and the mixture distributed wholesale, gratis. Depôts were formed in easily accessible spots in those places where the epidemic raged worst, at which quinine was stored. The Cape Mounted Police, in whose charge these depôts mostly were, made up the mixtures (after instruction by the District Surgeon), and distributed them gratis. Other members of the Cape Mounted Police (whose gratuitous work in this connection is deserving of more than passing mention) distributed thousands of tabloids on every patrol, and gave the necessary instruction *re* dosage where required. In this manner over eight hundred ounces of quinine (sulphate or bisulphate) and thirty thousand tabloids were distributed gratis in this district alone.

The great difficulty was to get people to continue the treatment after the actual malarial attack was past. Even the best educated refused very often to continue the use of a nauseous drug longer than absolutely necessary. They openly preferred the disease to the remedy, and the various village quacks reaped a golden harvest retailing vaunted specifics at exorbitant prices.

With regard to the *salts of quinine* used by myself personally, I preferred the *bisulphate,* which is readily soluble in water, and of a specific action equal to the sulphate. It is, however, dearer than the latter. *Euquinine* was most useful for children on account of its being tasteless. It is, however, too expensive for general use. I may say here that my experience is that patients of all ages as a rule take quinine mixture readily. I have given it to patients whose ages ranged from two months to eighty years. With children I always advised the horizontal position while administration was effected, and for a few minutes afterwards. If vomiting occurred, the dose was repeated in half an hour. I never found that the drug was vomited more than twice.

Methylene blue I used in a few cases of tertian ague, but not sufficiently often to be able to compare its action with that of quinine. It seemed successful.

Warburg's Tincture was used during a dearth of quinine, due to my inability to obtain sufficient supplies from Cape Town chemists. (The cultivation of cinchona trees and the manufacture of quinine in South Africa would be worthy of consideration.)

Arsenic I used mostly in conjunction with iron as a tonic in cachetics and those suffering from post-malarial anæmia.

Dimethyl sodium arsenate I used in one case of pronounced cachexia after everything else seemed to have failed. It was disappointing.

Of *atoxyl* in malaria I have no experience.

With regard to the routine treatment of severe cases, where possible, I began in adults with an intramuscular injection of quin. bihydrochloride, giving five-grain doses of the bisulphate orally four times daily at the same time; this was continued for one week, and then afterwards a ten-grain dose was ordered once a week after a dose of Epsom salts. The weekly dose was continued throughout the malarial season, and a similar course of weekly doses suggested in subsequent years during these months. This routine treatment gave good results, although, as I have said before, the difficulty was to get people to persevere, and recurrences were the result of their carelessness in this matter. Despite the verdict of several authorities to the contrary, I got good results from the intramuscular use of quinine. It was used in all cases where the drug could not be tolerated by the mouth, or where it was necessary to get the patient under the influence of the drug quickly, as in comatose conditions, hyperpyrexia, etc., and in those cases which, despite five-grain doses of quinine daily, still got paroxysms at intervals. In the latter class of case I found that a daily injection of nine-grains of the bihydrochloride, stopping the drug by the mouth completely, which was of the greatest benefit. I cannot reconcile the statements of other observers that these injections are "dangerous" (causing abscess and tetanus), or that the drug is "absorbed very slowly," with my own experience. I have given over one hundred injections with no bad results in any way. With regard to the technique, it is too well known to be detailed in this paper. It suffices to say that surgical asepsis is required in patient, operator, and instruments. The all-glass syringe used was boiled before each injection, and after withdrawal of the needle the puncture was painted with a ten per cent. spirituous solution of iodine and covered with a sterile dressing.

For the distressing headache accompanying the paroxysms I found ten grains of phenacetin with caff. citrat. of great help. It seemed to induce earlier and freer sweating.

For cases of æstivo-autumnal type with foul breath, furred tongue and quasi-typhoid symptoms, the mist. chlori cum quinina (Burney Yeo) was most useful, while for pneumonia complicating malaria I got the best results with small doses of quinine in effervescence (Burney Yeo). Very often, too, when other quinine mixtures were vomited persistently this effervescent mixture proved successful.

COMPLICATIONS.—As reported by Dr. Theron, of Kakamas, an infinite variety of complications accompanied and followed malaria. Of those more commonly met with I shall instance—

Skin eruptions, such as herpes labialis and herpes zoster, the former being met with in quite eighty per cent. of cases during the attack, the latter being a later trouble, rarely seen. Erythematous, urticarial, papular and scarlatiniform rashes were frequently met with. Theron describes cases of dermographia, miliary rash, and an "alarming case of œdema glottidis." All my cases required no local treatment, the general quinine treatment proving sufficient.

One case is worthy of note. It was that of a "young man about town" who presented a rash typical of secondary syphilis, which almost led me to commit an error in diagnosis as unfortunate for myself as disconcerting for my patient. However, the lack of history and absence of other specific symptoms and physical signs, and its complete disappearance, as suddenly as it came, with efficient quinine treatment, cleared up the doubt as to the nature of the rash.

Septic sore throats of every degree of seriousness were common complications. They were probably accounted for by the foul gases arising from the decaying vegetable matter left under a burning sun after the subsidence of the floods. These foul exhalations were thought by the laity to be the cause of the epidemic, as was the malaria, which gave its name to the disease, by the earliest writers on the subject.

True diphtheria broke out at various points along the river, and on the farms in the back-country.

Epistaxis was occasionally a serious complication.

Neuritis, particularly of the sciatic, intercostal and supra-orbital nerves, was a complication in fully fifty per cent. of cases. It often persisted and relapsed long after the epidemic had died out.

Pneumonia and other lung troubles were rarely met with during the epidemic, but some recent cases may be traced to the anæmia and bad general health of the poor following the malaria.

Abortion was liable to occur in the early weeks of pregnancies commencing during the epidemic. Women in the later months of pregnancy who contracted malaria were treated with small doses of quinine only so long as the fever lasted, but after their confinements they were, if accessible, immediately put on a thorough course of the drug, so that I really saw very few cases of malaria during the puerperium.

Acute nephritis was frequently met with, especially after cases of æstivo-autumnal type.

RARER COMPLICATIONS.—Two cases of neuritis of the brachial plexus were seen, followed by great wasting and loss of power of the muscles of the upper arm and forearm.

Two cases of paraplegia were seen, in children, due to toxic peripheral neuritis.

Theron reports a case of general pain all over the body, in a man aged fifty, which ultimately settled in the right arm, which became very swollen and the skin glistening. In three months' time there were nodules along the course of the median nerve, with extreme tenderness in the region of the brachial plexus, and marked atrophy of the deltoid and total inability to lift the arm or move the fingers. Treatment by anti-rheumatic remedies was of no avail, morphia having to be used to quiet the pain. Ammonol, five grains, *ter in die,* and potassium iodide were used with more success in the later stages.

He also reports a remarkable case in a woman, aged forty-two years, who, during an attack of malaria, after severe pain in the anterior triangle of the neck, suddenly developed a firm swelling of the size of a hen's egg, the exact shape of an enlarged left lobe of the thyroid. The swelling moved with the trachea on swallowing; there was no fluctuation, and a souffle was distinctly audible over it. On recovery from the malaria the swelling disappeared in two months' time as mysteriously as it came.

I myself treated two most interesting cases with the following history: Severe attacks of the æstivo-autumnal type of malaria were experienced by them in June and August, the latter being complicated by acute nephritis with albuminuria, casts, scanty urine, etc. When seen in mid-September, the feet of both were enormously swollen, and the skin of the toes of the left foot at the interphalangeal joints was so ulcerated that they were hanging by little tags of tissue, and were painlessly removed by a few cuts of the scissors. One patient told me that in his attempts to walk, the skin was so swollen that it cracked of its own accord, and the cracking had gone on until it had produced the condition described. The separated phalanges were blue, cold and practically dead. The proximal ulcerated surfaces were covered with healthy granulations, which ultimately healed and left sound stumps. What had probably happened was that the great œdema had induced a condition in which the capillary circulation had become stagnant, this being followed by localised gangrene of the terminal phalanges. The cracking of the skin had gone on to deep ulceration, until all the tissues had been eaten through and practically spontaneous amputation of the toes resulted. The condition simulated lepra mutilans, but in neither case were there any symptoms or physical signs of leprosy. One case had suffered for years from tertiary syphilis, and chronic endarteritis may have had something to do with the condition, but, in the other, I could not find any trace or history of syphilis. Both patients were coloured, male adults, in very indigent circumstances. They both ultimately recovered, although one died later of acute pneumonia. Professor Osler reports a case of multiple gangrene following malaria, but, as there is no detailed account of the case in his text-book, or in any other literature on malaria that I have read, I have ventured to give fuller details of this complication.

The above list could be lengthened indefinitely by mentioning cases of dysentery, diplopia, epilepsy, convulsions, neurasthenia, hysteria and even insanity, all of which diseases were seen as complications of the epidemic under consideration, but sufficient has been said to show the wide range of the sequelæ met with during the outbreak.

Time alone will show whether the growing habit of dating various ailments and chronic ill-health from the epidemic is correct. One cannot help being astonished at the number of people who have been crippled in health apparently as a direct result of the outbreak.

In view of the opinions of a recent writer on malaria who attributes a remarkable increase of crime in his—previously non-malarial—district to a recent virulent outbreak of this disease, it will be of interest to note the late effects of the 1909 epidemic along the Orange River Valley on the moral and intellectual character of the inhabitants dwelling in these parts.

IX.—THE URGENT NECESSITY FOR A MIDWIVES ACT IN SOUTH AFRICA.

By Dr. Frederick C. Willmot, Vredenburg, Malmesbury.

In all great social reforms the medical profession have generally been the pioneers, but that we have got so far in matters medical as to start chairs in Anatomy and Physiology at the South African College and yet do not possess a Midwives Act seems incredible.

In South Africa the evils of custom and ignorance are rapidly succumbing to education and greater enlightenment, and the time is ripe for some form of compulsory registration of midwives throughout all urban and rural areas.

Dr. Gregory, in his Annual Report for 1908, refers to the matter thus:—"At present hackney carriage drivers and night-soil men are generally required to be licensed, but any dirty, ignorant, half-blind, half-coloured woman can jeopardise the lives of lying-in women without restraint."

As the matter now stands there is no encouragement for a midwife to undergo the ordeal of an examination qualifying for registration, for such registration is not compulsory.

Under the Medical and Pharmacy Acts, midwives are to a certain degree under control of the Medical Councils, and under the Public Health Amendment Act of Cape Colony midwives may be brought to book in certain cases of puerperal fever, but only with very great difficulty. The result is that the loss of life (that of mothers and infants) in South Africa caused by the unskilled and neglectful treatment by midwives is simply appalling.

Every medical man in practice knows this, and he knows more: he knows that a large number of the uncertified midwives even do a trade in illegal operations, and the law does not touch them.

Vital statistics can give no adequate idea of the subject, but the following may be of interest:—In the years 1908 and 1909 in Cape Colony alone 530 deaths (174 whites) occured after child-birth, and 272 of these deaths were uncertified by any medical man. During this same period 123 deaths occurred from puerperal fever in Cape Colony, and the number of still-births in Cape Colony for the two years 1908 and 1909 actually totalled 1,996 (561 whites). These figures are inadequate, but are they not astounding?

To show how impossible it is to obtain a conviction against an unskilled midwife, I quote the following glaring example from the Annual Report of the Medical Officer of Health of the Cape Colony for 1905. At the end of 1905 a case of fatal puerperal fever occurred in the practice of a midwife carrying on practice in conjunction with her two daughters in the Cape Peninsula. On investigation, the following previous history of this midwife was recorded:—Towards the end of 1901 this midwife attended a coloured woman in Cape Town, who died of puerperal fever. In September, 1901, the Resident Magistrate of Cape Town held an inquest on a European woman who was attended by this midwife. In October the Resident Magistrate of Cape Town held another inquest on a woman and her child, who were attended by this midwife. In the middle of 1904 this midwife had another fatal case. About the end of the same year this midwife again had a case of puerperal fever. The next year, 1905, a European lady developed puerperal fever in the midwife's house. She was subsequently removed to hospital, where she died. A little later a European woman at Woodstock also died of puerperal fever at the hands of the same midwife.

In spite of this record of eight "suspicious cases," the midwife continues to practise untouched by the law of the land.

If such a state of things exists within the boundaries of the Cape Peninsula, it is easy to imagine how much worse they must be in the more remote rural areas.

In my own few years of country practice several glaring examples of negligence on the part of the midwife have come under my notice, the most glaring one being that of an old coloured midwife practising at St. Helena Bay. I was called in to see a case of hers last year. It was on the tenth day, and the patient was just expiring, but with her last gasps she told me that the midwife had not washed her after the birth of the child, nor had she even removed the filthy clothing in which the patient was confined. It is not necessary to further describe the filthy condition in which the patient was lying, but I thought it my duty to inform the police of the facts. The midwife was summoned before the Assistant Resident Magistrate, Hopefield, but escaped with an insignificant sentence, and her reputation is now greater than ever. Now, supposing we do formulate a Midwives Act, what form should it take? Well, to begin with, it must be a very elastic Act. To bring in a *stringent* Act would simply mean an *unworkable* one, because the supply and demand would be unequal. The large towns possess a small supply of certified women, but the rural areas know them not. The first aim of the Act should be to get a large number of women (white and coloured) on the Register. Once they are on the Register a strong penal clause would control them and weed out the "wrong-uns." All women who could prove to the satisfaction of the Council or some central committee that they had been practising midwifery during the past five years, and who could produce evidence of good character, should be placed on the Register without examination. In the case of coloured midwives the Council's certificate should only entitle such midwives to practise amongst coloured people. (Aboriginal Natives should alone be exempt from the Act.) At the time of receiving her certificate each midwife would also receive a printed set of rules, which she would be expected to observe in all cases of confinement. These rules would embody such matters as the cleansing of the hands, the cleansing of the patient, the symptoms of sepsis, etc., etc.

With regard to women who could *not* be placed on the Register under the five-years' clause, I think, for the present at least, the examination for admission should be a moderately easy one. (In the case of a coloured woman in rural areas a *viva voce* examination at the hands of the local District Surgeon should suffice.) At such future time as the supply and demand would become equal the standard of examination could be considerably raised, *or* an amended Act could be drawn up.

The last point of my paper is the question of *penal clauses,* and I will just say that these clauses will have to be very stringent, and very carefully drawn up, for the whole success of the Act centres around them. The Council must have power to prosecute any woman who attends a lying-in case and who is guilty of negligence or misdemeanour. There must be no question of "title." The cancelling of a midwife's certificate must be made an easy matter, and it must effectively prevent her from further practice.

D.—SECTION OF OBSTETRICS AND GYNÆCOLOGY.

I.—THE DIAGNOSIS AND TREATMENT OF FIBROID DISEASES OF THE UTERUS.

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(Special Subject for Discussion.)

Although of necessity haunted by the uncomfortable knowledge that the subject of uterine myomata is very much better set forth in a score of text-books than in the very poor *résumé* to which time, as well as my own experience, limits me now, I shall endeavour to put before you as concisely as possible, in the first place the principal points on which the diagnosis of fibroid disease is based, and secondly, the surgical principles involved in its treatment, together with a review of the measures best adapted to secure those surgical principles effectively.

The symptoms in fibroid disease, at all events those of chief importance, fall into two groups, which may be named respectively (1) Hæmorrhage, and (2) Tumour. A woman whose uterus contains one or more fibroids will come to her doctor suffering from one or both of these symptoms as the case may be or from none at all; but I need hardly point out that, in the latter case, before deciding not to interfere on the mere diagnosis of the condition, the surgeon should look ahead; preventive surgery is oftentimes quite as creditable as curative surgery.

I. Hæmorrhage.

Uterine hæmorrhage due to fibroids is usually of the type called menorrhagia, that is to say, an increase of the menstrual loss, often both in quantity and duration. It is somewhat curious that many difficulties have been raised over the explanation of this increase. The myomatous uterus bleeds more readily and freely than the normal because its mucous surface is enlarged by the fibroid; there is, in fact, a larger surface for the bleeding to come from. In this connection a perusal of the countless suggestions which have been made as to suitable methods of stopping the bleeding is interesting, but hardly creditable to the science or art of gynæcology; Apostoli, with his electrical treatment of the endometrium, is the best known of the non-operating enthusiasts, though by no means the most ridiculous.

Meno- or metrorrhagia is produced by a fibroid polypus; by a sessile submucous fibroid, or even by a strictly intramural or interstitial fibroid, provided that the latter causes enlargement of the uterine cavity. I do not say that every myomatous uterus with an enlarged cavity bleeds, but I say that, as far as I am aware, excessive menstrual loss does not occur unless there is some enlargement of the uterine cavity. It is fairly obvious that a subperitoneal fibroid, as it does not produce such enlargement, is not likely to cause menorrhagia.

Consider first hæmorrhage associated with intramural fibroids. The patient is typically a woman over 35 years of age who has had several children, and who complains that her periods have recently become excessive; the flow is increased and usually lasts longer, while the time between the periods has diminished. An examination shews enlargement of the uterus, varying from an apparently uniform increase in size to that well-known condition where the tumour seems to be made up of multiple knobs fused together. The sound usually passes from three and a half to five inches or more, though I need hardly remark that failure to pass the sound for more than two and a half inches does not necessarily mean that there is no enlargement of the uterine cavity. The diagnosis in such a case is fairly obvious; but as I shall have to point out with reference to differential diagnosis, the need of care in the thorough examination of such cases cannot be overrated. Let me remind you of two aphorisms—firstly, don't pass the sound without otherwise excluding pregnancy; secondly, don't jump to the conclusion, because one disease is obviously present, that there cannot be any other. I have seen a supravaginal amputation on the point of being performed for an undoubtedly myomatous uterus, when the cause of the hæmorrhage was fortunately discovered to be malignant ademona of the endometrium, necessitating a modification of the proposed procedure.

An intramural fibroid causing few or no symptoms may undoubtedly remain, if it does not grow rapidly, *in statu quo* for a considerable time, but the tendency is for it to become, as the result partly of its own growth and partly of the extruding action of the uterine musculature, more and more submucous. It is not, however, as I have already pointed out, the degree of nearness of the tumour to the endometrium which determines the amount of bleeding, but the degree of enlargement of the mucous surface produced by the tumour, whether submucous or interstitial. That is to say, the expression "bleeding fibroids" is inaccurate and misleading; fibroids do not bleed. Once in the submucous position, fibroids are very apt, especially after labour or abortion, to "slough," that is, to become extruded as a more or less disintegrated stinking mass, and so expelled from the vagina. Quite recently a patient of mine, who had been attended by a midwife, gave me an interesting account of the evacuation of a sloughing fibroid thirteen days after delivery. A foul-smelling, tangled mass appeared one day, which the midwife disconcerted no doubt, but in no wise daunted, stoutly declared to be a "false conception," and (quite rightly, as it happened) douched her patient daily until the fibroid had escaped. Subsequent examination revealed the existence of numerous other fibroids. This condition is somewhat different from the "necrobiosis" which so commonly occurs in pedunculated subserous fibroids, to which we must refer later.

When Resident Accoucheur at the London Hospital, I once admitted to the gynæcological ward a Jewess of 40 in a pulseless and moribund condition. Examination showed a rounded tumour of about the size and consistency of a small orange occupying the upper part of the vagina, from around which blood was oozing. She died in five minutes before saline infusion could be performed. It may be well, before leaving this subject, to refer briefly to the differential diagnosis of fibroid polypus. Such a polypus while still within or above the cervical canal can only be diagnosed certainly by dilating the canal. When it comes to occupy the upper part of the vagina, diagnosis is not difficult if the sound be used in the manner described in text-books, where laborious comparisons are usually drawn between fibroid polypus and chronic inversion. Now I have seen three cases of chronic inversion (one within the last six months), and none of them in the least resembled a fibroid polypus. The inverted mucosa, besides being readily recognisable by bimanual examination, presents a characteristic boggy, œdematous conical mass, on which stands out clearly the peculiar sweat-like secretion from the mouths of the uterine glands rapidly renewed after sponging the surface dry; further, the Fallopian orifices can generally be distinguished. What, however, is liable to cause trouble to the unwary is the not infrequent association of chronic inversion with the polypus. Such an association in the case of the bowel, *viz.*, a polypus heading an intussusception, is well-known to abdominal surgeons. I remember a co-resident of mine starting to remove an innocent-looking polypus with scissors, and rapidly discovering that he had opened up the peritoneal cavity, a fact which subsequently necessitated a hysterectomy. I shall have to refer later to the treatment of fibroid polypus, but we must go on to consider the other symptoms produced by uterine myomata, all or nearly all of which come in some way under the heading—

2. TUMOUR.

The size at which a fibroid tumour will begin to produce symptoms depends, of course, on its particular relations to the uterus and adjoining viscera. I have seen a woman, for instance, a hospital nurse, who had a fibroid no larger than a hen's egg in the posterior uterine wall which necessitated operation because it produced persistent and troublesome retroversion. On the other hand, I removed some time ago from another patient, a hard-working nurse in a children's home, a uterus weighing seven pounds, a mass of fibroids; the patient had noticed nothing but some increase of abdominal girth, which she had attributed to the "change of life." Interference with the functions of the bladder to a greater or less degree is usually a relatively early symptom. Most commonly there is inability to retain the urine for any length of time, and this is usually a true incontinence. In some cases, however, the condition seen is actually retention with overflow.

I once admitted a hospital patient four days after her confinement; she looked ill, had a temperature of 102°, and three distinct abdominal tumours. The catheter dispersed the first one, while operation proved the other two to be respectively the uterus and a large subserous fibroid undergoing softening in its interior. A hysterectomy was followed unfortunately by death. I fear it is a digression, but it is worth while making a few remarks in passing about this curious and not uncommon condition of "white softening" to which large fibroids are liable, and to which the name "necrobiosis" is applied, because it opens up a very interesting subject to which a good deal of attention has been drawn lately, *viz.*, the physiology of uterine involution. It has long been known that necrobiosis is much more prone to occur in connection with child-birth or abortion, and the bearing of this on uterine involution was ably discussed about six months ago in the *British Medical Journal* by Dr. Langridge. In his paper he advocates a theory, not quite a new one, that the rapid loss of weight undergone by the uterus in the early days of the puerperium (amounting to one-half during the first week) is due to *autolysis*—the production, that is to say, of certain bodies, possibly sarcolactic acid among them, which actually dissolve the muscle substance. In a normal child-birth, he says, the uterine blood supply is absolutely shut off for the first week, owing to the occluding mechanism of the uterine arteries; during this period the *lysin* bodies are formed, and when, at the end of the week, the normal blood supply is somewhat suddenly restored, the dissolved waste products are washed out of the uterine muscle, and a sudden rapid rise in kreatinin excretion occurs in the urine. In a few rare cases the blood supply is not restored, and then we get that curious condition known as puerperal atrophy, indicated by continued diminution in size of the uterus and complete cessation of uterine functions. I refer to this theory here because it may hint at some explanation of the occurrence of white softening in fibroids. A softening of a somewhat different kind (often called "red" degeneration) is apt to occur in subperitoneal fibroids, and is due to twisting of the pedicle of the tumour; the veins, being the larger and softer, are compressed first, the arteries feeling the effect later. The symptoms produced by the softening of subperitoneal fibroids are, on the one hand, those of a mild degree of peritoneal shock—that is to say, chiefly pain and tenderness—and on the other those due to toxic absorption. Clinically, in the one case the patient, who has probably had a large fibroid for some time, complains that the tumour has latterly become somewhat larger and distinctly tender; or, again, her symptoms may be much more acute than this, and may even have come on with remarkable suddenness, the condition suggesting on examination the typical "ovarian with twisted pedicle," from which the diagnosis has to be established.

In the other case the tumour may appear very much as it has done for months past, but the patient *looks* ill; her tongue is furred and her temperature a little raised, though, apart from some tenderness on examination, there may be very little about her to draw particular attention to the tumour. Such symptoms are suggestive of "softening" of the fibroid, but there is again another condition altogether to which some reference must be made here, as it is not uncommon, *viz.*, "cystic degeneration" of fibroids. Suffice it to say that the symptoms, at all events as regards any change in the tumour itself, are usually negative, while physical examination shows a large fluctuating tumour which the surgeon, unless he be helped by finding an increase in size of the uterine cavity, may be pardoned for mistaking for an ovarian cyst.

To return to the question of pressure symptoms, it is seldom that the bowel functions are very much interfered with, owing to the power of the rectum to adapt itself to altered conditions. Pressure on the sacral nerves may cause trouble, but does not often do so; swelling of the lower extremities from pressure on the pelvic veins is more common. Pressure on the ureters is fortunately not very common, but is serious when it does exist, chiefly because it is apt to complicate the prognosis, as regards operation. Ascites may occur, and, curiously enough, in connection with comparatively small tumours.

Differential Diagnosis of Uterine Fibroids.

It is well to impress upon one's mind at every opportunity, whether in hospital work or in private practice, that the commonest abdominal tumour in women is the pregnant uterus. Therefore it must be our duty, in every case of supposed uterine fibroid or other new growth, to exclude pregnancy first. The fact that most eminent abdominal surgeons have in their time been trapped into unintentional operations on the pregnant abdomen must be my excuse for suggesting here one or two precautions which may save us from this unfortunate blunder. The first is never to operate on someone else's diagnosis. The second is to examine one's patient thoroughly and systematically in good daylight—breasts, abdomen, and vulva—and finally to make careful examination of the pelvic contents. Note the condition of the breasts, and whether there is blueness of the vaginal mucosa; the latter is sometimes present with fibroids, but usually only when they are large. Do not pass the sound unless you can exclude pregnancy, and don't forget the possibility of pregnancy *with* fibroids. The most difficult case of all is apt to be a fibroid uterus containing a dead ovum—that is a "missed abortion." But it may fairly be said that if one's examination be conducted with an eye to the possibility of pregnancy, mistakes will be unlikely. The fibroid uterus is usually harder than the pregnant uterus; softening of the cervix, and more particularly the sign called Hegar's, do not occur with fibroids, nor does ballottement; nor is a fœtal heart audible, though a fair imitation of a "souffle" may be met with.

I have taken pregnancy first because of its importance, but, strictly speaking, the first question which should arise in the case of a tumour of pelvic origin, whether associated with bleeding or otherwise, is whether the tumour is uterine or ovarian. This is not always a very easy thing to determine, as, on the one hand, a fibroid (especially a cystic one), owing to its pedunculated character, often gives the impression of being entirely distinct from the uterus, while, on the other, a large tense ovarian tumour may so press or pull on the uterus that it seems to be closely united with that organ.

As regards the question of innocence *versus* malignancy, the only point likelv to give much help short of operation is the history, especially as to the rapidity or otherwise of the onset of symptoms. I am speaking now mainly of subserous myomata. Formerly, even at the time of operation, no distinction seems to have been attempted; it was enough to know that in certain cases the growth recurred after operation, and then the case was called one of "recurrent fibroids." Relatively these cases of sarcoma of the body of the uterus are not very common, but they are quite common enough to constitute a very strong reason why every "fibroid" (generally speaking) should be removed by operation, because it is quite certain that there is no certain means of distinguishing typical from atypical uterine tumours before operation. Even laparotomy may leave the matter undecided, though, as far as my own experience goes, it is not generally very difficult to tell the difference when the abdomen is opened. The general appearance of the tumour, its softness and vascularity, its lack of proper capsule—these points will generally make diagnosis possible, or at all events lead the operator, in doubtful cases, to make more certain of his result by a complete hysterectomy. In connection with possible malignancy, probably the most important aspect of our subject, I am quite sure that the very greatest care is necessary in discriminating between fibroid disease of the uterus and cancer of the endometrium or cervical canal. A great deal has been said and written about the frequency with which fibroids are associated with, or their removal followed by, carcinoma; in fact, not a few have gone so far as to say that supravaginal amputation of the uterus, which for reasons I shall mention I consider the best routine operation for uterine fibroids, should be given up because cancer of the cervix follows so frequently. Now fibroid disease is common, and so, alas! is cancer; hence it is not surprising that the two should co-exist somewhat frequently, and there is not much doubt that the supervention of cancer in the cervical stump after removal of fibroids is in reality either a mere continuance of a pre-existing malignant adenoma or (as in one case which I saw) of a squamous carcinoma, or a downward extension of a cancer of the endometrium "scotched but not killed" by a too partial operation. It is scarcely likely that a cancer starting *de novo* would find a suitable nidus in the functionless and devitalised cervical stump, and cancer is a new growth, not degeneration.

Treatment of Uterine Fibroids.

Should every case of fibroid disease of the uterus be submitted to operation? With the understanding that certain circumstances may alter certain cases, I take the liberty of saying "Yes," and a comparative study of the various recently expressed opinions throughout the world leaves no doubt as to the verdict of modern surgeons, while a due consideration of fundamental surgical principles leads to the conclusion that the vast majority of cases demanding any treatment at all (to put it that way) can only with propriety be treated by operation. It is quite clear that the one great and to my mind sufficient reason which has prompted this universal change of opinion is the fact that there is no certain means of distinguishing before operation between fibroid and sarcoma. I have said before that in general a sarcoma is distinguishable at the operation, but at the same time there are certainly cases met with in which what looks like a fibroid will shew sarcomatous features on section, and will recur after a partial operation; such a tumour will of course present the clinical features of a fibroid, and if treated expectantly, will inevitably go too far for operation to give the patient a chance. This discussion will have done some good if it should replace in some of our minds the older dictum, "Do not operate until you are obliged," with something more like "Do not put off operation unless you are obliged." There are reasons why it is necessary to touch on the non-operative treatment of fibroids, though one can dismiss very shortly the countless forms of palliative treatment invented by ingenious people who really shirked the operation, and even when they did operate were inclined to substitute supposedly palliative operations for the radical one.

1. Non-operative Treatment.—In the first place, the expectant treatment of fibroids was based on the supposition, correct in some cases, that fibroids cease to grow, or even atrophy, after the menopause. The object of treatment was twofold: (1) To stop the hæmorrhage; (2) to cause absorption of the tumour. The first to

emphasize and systematize the use of ergot for this purpose was Hildebrandt in 1872. He gave it hypodermically, but probably it matters little how the ergot (or what derivative be used) is given, provided it be exhibited continuously for some months. There are certainly many cases in which the prolonged administration of ergot is of great value. My own practice is to give it in the form of a pill containing two grains of ergotin thrice daily; where ergot fails other hæmostatic drugs are not likely to succeed; in this particular connection, therefore, we need not discuss them.

Applications to the Endometrium.—As Dr. Herman pithily remarks, the endometrium bleeds, not the tumour. Hence, if you wash out the uterus with a styptic, or scrape and cauterize the endometrium, bleeding is checked for a time. Such proceedings can hardly even be called palliative. Mention must be made in passing, as it has a place in history, of the electrical treatment associated with the name of Apostoli. Apostoli stated that he produced "an electrolytic action by which the absorption of the tumour *may* be brought about." The only really obvious effect is a scorching or cooking of the endometrium, which presumably stops the bleeding for a time. Stuart Nairne, who published an analysis of 106 cases in 1891, found the result "good in 28, indifferent in 13, bad in 21, very bad in 44." There is no evidence that I know of to shew that the method is anything but wrong in theory and bad in practice; at all events, its results, as Herman remarks, are far inferior to those of ergot.

2. Operative Treatment.—The operative treatment of fibroid polypus may be dismissed first. As a rule, if the polypus be diagnosed before it has reached the external os, it is not only safe, but often advisable, to give ergot and wait, but in case of any reason for hurrying the process of expulsion, it is a simple matter to dilate the cervical canal and extract the tumour. For such dilatations I prefer to put in a laminaria tent or two the previous evening; the process of dilatation is thus rendered more rapid the next day under the anæsthetic, and it is easy to remove the fibroid with curved scissors and curette the remains of the pedicle. A polypus which has reached the vagina should be removed with as little delay as possible, because the lower it gets in the vagina the more does infection from without come into play, the tumour coming to present a sloughy-looking surface and texture, a condition which I am quite sure renders the otherwise simple enough operation of removal more risky than is usually supposed. I remember seeing pulmonary embolism occur in two cases in hospital some years ago within a short period of each other; in the one case death occurred on the table, in the other about an hour after operation. Thorough antiseptic douching is advisable both before and after the operation, and as I have said before, extreme caution is necessary to avoid injuring the possibly inverted fundus uteri.

As a general rule, I do not think any other kind of fibroid should be extracted *viâ* the vagina, whether with or without the uterus.

Abdominal Section for Fibroid Disease.—It used to be the custom to advise in the case of large myomata, the removal of the tubes and ovaries only, with a view to producing an artificial menopause and the consequent shrinking of the tumour, because it was supposed that such an operation was less dangerous. It is quite sufficient to cite two of several reasons why this was a bad procedure; firstly, it violates an important principle of surgery, *viz.*, the removal of diseased structures and these only. Secondly, the patient is being unsexed with exceedingly doubtful prospects of any benefit at all, and oöphorectomy for fibroids is no more reasonable than is double castration in the male for enlargement of the prostate. Nowadays the tumour is removed, with or without the uterus, and every effort is made to preserve at least one ovary. Speaking generally, only two methods are now recognised of dealing with myomata exposed by laparotomy; the one is incision of the uterine wall and "shelling out" of the tumour, the other the removal of the whole, or nearly the whole, uterus containing the tumour. The credit for the introduction of the former method belongs to Martin, and the operation calls for a brief reference here, as it has been extensively practised. At first sight it would seem to be a much more perfect application of the surgical principle to which I have just referred than hysterectomy, but in my opinion this is fallacious. The principal objections to it are: (1) that it is more dangerous than hysterectomy, especially when applied to large tumours, and (2) that it is apt to be unsatisfactory in its results owing to other fibroids remaining behind. It is enough to remark that the following are the essential points of the operation needing attention:—

(1) Cut right down into the proper capsule of the tumour, otherwise bleeding will be excessive;
(2) Do not open the endometrium; and
(3) Close the cavity by suture just as carefully as after a Cæsarean section.

I do not like the operation, and can see no advantage possessed by it. I have seen at least two cases in which a hysterectomy was subsequently called for, and surely insufficiency is quite a serious enough indictment of any operation.

Statistics seem to shew that supravaginal amputation of the corpus uteri is the safest operation all round for the removal of the fibroid uterus; it is certainly somewhat quicker and easier than complete hysterectomy, or, as it is needlessly termed, *panhysterectomy*, besides which it has several other well-defined advantages. It results in less deformity, the relation of the cervix and vagina being unaltered, so that in some cases it is not very easy to tell later on that the uterine body has been removed, while there is certainly less risk of injuring the ureters. The only objection, as far as I know, lodged against this operation which could, if well grounded, have any weight is that there is a risk of cancer of the cervical stump supervening. If we push this argument to its logical conclusion, we ought to remove every woman's uterus as a matter of routine after she has had a child or two, for there is, as I have already submitted, but little evidence that carcinoma is any more likely to attack the cervical stump left after amputation than the normal cervix. The cervix should be removed with the body in every case—

(1) Where this is the easier procedure;
(2) Where there is the slightest suspicion of malignancy in the supposed "fibroid," the endometrium, or the cervix.

Supravaginal Amputation of the Uterus.—The steps in the operation, as usually performed, are as follow. Personally, I start with the patient in a full Trendelenburg position, use the vertical median incision, and always wear rubber gloves, as one is then freer to do the necessary handling of the viscera. The large gauze swabs, which alone are allowed to enter the abdomen, must be counted both before and after the operation. The incision is made long enough to expose the uterus and adnexa thoroughly, and the uterus is lifted with a strong vulsellum, which in most cases is better than a "corkscrew." The left round ligament is tied, clamped on the uterine side and cut; then the left ovarian and tubal vessels are similarly treated, it having been decided first whether one or both ovaries are worth preserving; the ligature is placed accordingly. Next, I ligature, clamp and cut the right round ligament, and with this guide cut the peritoneum across the front of the uterus from ligament to ligament; the vesical fold is then separated by blunt dissection downwards. The right ovarian and tubal vessels are now tied and clamped, and divided, after which it is usually possible to raise the uterus practically out of the pelvis by traction, and the left uterine vessels can be exposed with ease. These are encircled by a rather blunt curved needle on a handle carrying stout catgut and tied, those of the right side being then similarly treated. A transverse incision is now made across the back of the cervix, dividing the peritoneum, but as it is seldom possible, on account of the closer adhesion of the peritoneum in this region, to turn down a peritoneal flap, it is usual to make this incision the beginning of a wedge-like amputation of the uterine body. Some carry this incision straight across the cervix just above the level of the ligatured uterine vessels; others, though the difference is probably immaterial, cut out a wedge of cervix to make flaps, which can be approximated by a few sutures, and so tend to check any hæmorrhage, which, however, is seldom troublesome. In any case, it is unnecessary to sterilise the cervical canal, as Herman pointed out long ago; if the operator does not like the look of the mucous membrane, it is better, as Kelly recommends, to dissect down and remove as much of it as possible. It is now always possible to cover in both the cervical stump (with its "flaps" approximated) and the raw surfaces of the broad ligaments by a continuous infolding catgut suture, the object of this being, of course, to leave nothing to encourage the formation of adhesions, and it is well to follow Kelly's plan of stitching the divided round ligaments to the cervical stump, which is thus kept suspended in a more natural position. The toilet of the peritoneum is now completed with swabs either dry or wrung out of normal saline solution. I think one may well assert sweepingly that no drainage of any sort should ever be used. The abdominal wall is sutured in layers entirely with catgut; the peritoneum first with fine catgut, then the fascia of the anterior sheath of the rectus, and finally the skin, the latter either with a subcutaneous catgut suture or the "blanket" stitch of continuous catgut, which I have always found effective. The use of silkworm gut is scarcely necessary; the proper healing and subsequent security of the abdominal wound depends, I believe, not on the durability of the suture material, but on the accuracy of apposition first of the peritoneum, then of the fascia.

I am glad to see that Kelly and Cullen allow their patients as much water as they like after the operation. Morphia is much better avoided if possible, as it does but little to relieve such discomfort as may follow the operation, which is chiefly due to flatulent distension. Some years ago I was greatly struck with the post-operative progress of a very long series of cases which I had the opportunity of observing in the wards of the London Hospital. In those days the patient was frequently under the anæsthetic for three hours, either the A.C.E. mixture or chloroform being used, always to very deep narcosis. I never saw an example of anything like severe shock; vomiting was the rare exception, and very little pain ever followed, morphia never being given. My observations as regards ether anæsthesia have not impressed me favourably. My own practice is to administer frequent saline rectal injections and one-sixtieth of a grain of strychnine hypodermically every four hours for the first night, the catheter being used four-hourly for the first twenty-four hours. Moynihan gives the useful hint that aspirin, either in 10-grain doses by the mouth or 20-grain doses *per rectum* (in mucilage), is very effective in relieving discomfort after all abdominal operations. Should flatulent distension not yield to saline enemata, it can be relieved by periodical passage of the long rectal tube, and an occasional enema either of 2 ounces each of glycerin and water (Kelly) or of half a pint of soap and water or gruel with an ounce of turpentine. I usually begin, provided there is no vomiting, with a little calomel on the second afternoon, giving half to one grain every hour up to 3 grains. I am strongly in favour of letting the patient begin to move about and sit up for a while in a chair from about the fifth day, but in this respect each case must be treated on its merits. If the incision has been a large one it is better, I think, to let the patient get a surgical belt and wear it until it is worn out, otherwise she may suffer later on from divarication of the recti through no particular fault in the operation, especially if there has been long-standing over-stretching of the abdominal walls.

DISCUSSION.

DR. ROGERS, after thanking Dr. Maxwell for the excellent introduction he had given to the discussion, referred to the question of myomectomy. He mentioned a case in which an operation was performed by him for a fibroid on the posterior wall of the uterus. The patient was pregnant and, although the cavity of the uterus was opened, the pregnancy was not interfered with and a full-time child was born. He thought that myomectomy might be employed in fibroids causing symptoms during pregnancy. He agreed with Dr. Maxwell in the good results following the use of aspirin given *per rectum* in 20-grain doses after abdominal operations. It could be combined, if necessary, with morphia hypodermically.

DR. WATERSTON stated the case from the women's point of view. She emphasised the dread which a woman has of loss of the genital organs, and made a strong plea for conservative measures.

DR. SIMPSON WELLS thanked Dr. Maxwell for his valuable introduction to the discussion, and agreed with most of his conclusions. There were one or two points on which, perhaps, there was room for difference of opinion. One was the question of myomectomy. He thought that in certain suitable cases this was the best treatment. In young women with subperitoneal fibroids, especially if pedunculated, myomectomy was a satisfactory operation. He referred to a case in which he had removed several myomata by the abdominal route, in which the patient had remained free from symptoms which had previously been severe. There was the danger, as shewn by Kelly's and Cullen's cases, of leaving fibromata behind. With reference to the question of operating on a myomatous uterus because of the danger of the tumour becoming sarcomatous, he thought that pressure effects, hæmorrhage, the danger of necrobiosis, and of pregnancy complicating the condition were more immediate reasons for operation.

Dr Wessels held that myomectomy was a very valuable operation in the treatment of fibroids, that during the child-bearing period the uterus should never be sacrificed when it was possible to enucleate the tumours. Sterility is frequently relieved, and patients therefore more readily submit to myomectomy than to hysterectomy. In older women with troublesome fibroids, one should not hesitate to remove the uterus, rather than wait for the menopause, which seldom, if ever, gave complete relief. In operating on a case of adenocarcinoma of the uterus four years ago, it was found that the growth was confined to the endometrium, round a very small calcified fibroid, and it appeared as if that were the exciting cause of the cancer.

II.—SOME THOUGHTS ON SEPSIS.

By A. H. Watkins, M.D. (Edin.), M.R.C.S. (Lond.).

In the Report of the Medical Officer of Health for the Cape Colony for the year 1908, occurs the following:—

"During the year, 67 cases of puerperal fever were reported. . . . The actual deaths certified were 35. . . . Undoubtedly a good many cases of this disease escape notification."

Standing as I do to-day on the threshold of political life, I hope I may be forgiven if I yield to the temptation to indulge in a sort of retrospect, and give you some "thoughts" on sepsis—with special reference to puerperal sepsis—as they have developed in my mind.

In the early seventies, when I was a student in Edinburgh, we had passed the stage of carbolic putty, and Lister was in the carbolic spray stage. A few surgeons had so far followed Lister, but only a few, and Lister's methods had as yet had no influence on obstetrics. Indeed, they could not have had, for, great as were to be the later developments, the theory on which those methods were based was still extremely crude. Practically no differentiation of the various forms of microbic life had yet taken place—all were lumped together in the one term, "germs." No indication of the defensive powers of the body tissues against these "germs" had yet been given. Though Lister was, later, one of the greatest pioneers in pushing his discoveries farther, and forming a correct estimate of the defensive powers of the tissues, at the time I am speaking of he had not recognised them, but held that total exclusion of the living germs with which the air was supposed to at all times and everywhere abound was the only means to prevent septic infection. His methods then were clearly inapplicable to midwifery.

Having washed our hands in Condy, it was felt that all that was necessary or possible had been done, and the lubricant we used depended largely on what the Fates supplied. Sweet oil was, of course, most often supplied, and *may* have been aseptic at the commencement of labour, but as little attention was given to the cup or saucer in which it was placed, as fingers removed from the vagina were freely dipped into it, as dust and flies accumulated on it, as labour progressed it could not have remained aseptic for long. The external genitals of the woman might have been clean, or might have been dirty; they usually were dirty, for the daily bath had not become a routine proceeding for women five-and-thirty years ago. For the accoucheur to have proceeded to cleanse them with soap and water would have been held to be the height of impropriety, for any exposure of the patient was held to be indelicate, and examinations were always made under the bed-clothes, students being taught to pass the catheter also under the bed-clothes by the sense of touch alone. The recognised guide for the examining finger in finding the vaginal orifice was the anus, and if a beginner put his finger into the rectum instead of the vagina, he was advised not to withdraw it hastily as if he had made a mistake, but to leisurely withdraw it, and then pass it into the vagina as if the two proceedings were but successive steps in one operation!

With such a technique as I have described (only the word technique was not then invented), it is not surprising that some cases became septic, ran a high temperature with offensive discharge, or developed tympanites and abdominal pain, with the classical signs of peritonitis—to be combatted by the classical and only remedy for peritonitis—opium.

What is surprising is that they did not all develop septicæmia and die. The mortality tables of thirty to forty years ago may not have been entirely accurate, but with regard to puerperal fever they probably were approximately so, as the nature of the disease was so evident that mistakes would not be numerous, and there was little temptation to conceal the cause of death, since puerperal fever was then regarded much as enteric fever or pneumonia is to-day, as a disease for which no one was specially to blame. The mortality returns varied from 1.4 to 2.9 deaths per 1,000 births, except in the year 1874, when they stood at 3.6 per 1,000, the highest point in sixty years—that is, taking the general returns for England and Wales; in some of the Continental lying-in hospitals it reached 30, 50 and even 90 per 1,000.

Leaving these institutions out of account, however, there is an interesting field for enquiry why, under the conditions I have pictured above (and I have not exaggerated the "septicity" of the usual technique), only 2 out of every 1,000 patients died of puerperal fever.

Of course, a very simple reply is that it shows the wonderful powers of nature, the excellence of the *vis medicatrix naturæ,* in the case of the 998, though this would still leave the problem unsolved of why nature failed in the other two.

Even to-day one so often hears "nature" spoken of as if it were an entity, a beneficent goddess charged with the care of the human body, and although the rack and thumb-screw are things of the past, one hesitates to commit the sacrilege of even suggesting scepticism as to "nature's" beneficence. More than one book I have dipped into of late, in deprecating meddlesome obstetric interference, urges that parturition, being a "natural" process, should be left to nature. Nevertheless, I am bold enough, or rash enough, to hold that though the conclusion may be sound, and unnecessary interference in labour be a thing to be deprecated, the premises do not warrant the conclusion drawn from them. As a matter of fact, "nature," to use the term for a moment as if it were an entity, cares nothing for the individual life.

Tennyson's words,—

> "So careful of the type she seems,
> So careless of the single life,"

are a great truth, only Tennyson might have left out "she seems," and have said "she is."

Granted that parturition is a natural process, clearly it must be admitted that the initial stage of procreation is also a natural process. Maeterlinck graphically describes the nuptial flight of the queen bee, followed by her successful lover, and how the act of copulation is followed by the immediate death of the male, whose spermatic organs are left permanently attached to the impregnated queen. Here nature seems not only careless of the single life; she seems to have deliberately sacrificed it to the continuation of the species.

Moreover, there is not the least reason to suppose that nature has any partizanship for one class of living organisms more than for any other class, and it seems to me that nature cares just as much for the microbe as she does for the man—or, to keep closer to our text, for the woman.

Man's whole life appears to be a struggle with inimical microbes—microbes of disintegration, microbes of disease. By some process, probably a process allied to what Darwin called natural selection, man has developed protecting powers which enable him successfully to resist these microbes under most circumstances, but at times his protecting powers fail, and the microbe wins—wins at times after a hard and prolonged fight in which for long the issue hangs in the balance, wins at times with an ease and rapidity which can only be described as cataclysmic! All of us can tell of "fulminating" cases, not only of appendicitis, but of cases in which, as a result of an injury which we should wholly disregard in filling up an insurance report, a patient has died in forty-eight hours or less. Why? Who of us can say?

To go back to the consideration of puerperal fever or septic infection in the parturient woman, what is the position fairly looked at to-day?

I remember attending the obstetric section of the British Medical Association meeting at Newcastle, I think in 1893, and listening to a paper and discussion on puerperal fever. We were all patting ourselves metaphorically on the back at the success of preventive treatment in our large maternity institutions. Douching with mercurial solution before and after labour was the panacea which was to abolish puerperal fever from our hospitals and from all maternity practice, and in future for anyone to lose a patient from septic infection was to be a disgrace, if not a crime.

Undoubtedly the new departure did much to reduce the mortality, especially in our lying-in hospitals—where thirty to forty years ago the death-rate was far higher than amongst women delivered in their own homes—but the high hopes that were raised of exterminating it altogether were doomed to disappointment.

Since then there has been a swing of the pendulum the other way, and we are now told that "douching, except in very special circumstances, is a thing of the past in properly-conducted maternities."

If we go outside our maternity institutions, and look at the death-rate from puerperal fever amongst the general population, we find that in the last thirty years there has been a distinct diminution in the rate of mortality, but still the deaths in England, Wales and Scotland in 1906 totalled 1,728, while in the Cape Colony in 1908 the *registered* deaths were 35. I regret I have no reliable up-to-date tables to submit to you, but the fact is incontrovertible that the diminution in the incidence of puerperal sepsis is very disappointing, and by no means on a par with that of typhus, smallpox, enteric fever, or even tuberculosis.

One has to remember that there is an undoubted tendency for the statistics of mortality from puerperal infection to be too favourable. It requires a great deal of moral courage on the part of a practitioner who has attended a case himself to hand the sorrowing husband a certificate stating that puerperal fever was the cause of death; and even when called in by the midwife to a septic case which he has not himself attended, where the nurse is known to him as a careful, respectable woman, ordinary charity leads him to look for some other reasonable explanation of the disastrous result. Many cases no doubt get certified as pneumonia, pleurisy, peritonitis, and so forth which had their origin in a puerperal septic infection.

Speaking just now of cataclysmic cases of septic infection, I raised the question as to the directions in which we should look for the explanation, and suggested two main lines of enquiry—the resisting power of the patient, and the potency of the attacking forces of the microbe.

Before the introduction of antiseptics, the resisting power of the patient received a considerable amount of attention, though by no means always wise attention: warm mufflings, heated rooms, stimulating food and drinks were apt to be more relied on than fresh air, cold water and exercise. With the first advent of antiseptics all attention was fixed on the attacking microbe, and the resisting powers of the patient were neglected. I well remember how, in the early days of enthusiasm and the carbolic spray, Lister scoffed at the idea of insanitary surroundings being a matter of any consequence to the surgical patient, provided only the omnipresent "germ'" could be killed by a large enough and well-directed spray before contaminating the surgical wound; and how all-important it was considered that the spray should be in full force before even the edge of a dressing was lifted in the after treatment of the wound, lest a single living germ should gain access. The old Royal Infirmary in Edinburgh was not an ideal building, and Lister's wards were always full to overflowing, usually, in the case of children, with two patients in the same bed, for excisions and erasions of tuberculous joints had with the diminished risk of operation become very frequent. Of course, the immediate gain was enormous. I dressed both in Glasgow and Edinburgh, under surgeons who had not accepted even the crude Listerian methods, and I have seen seven consecutive amputations, several of them only amputations of the foot, die of pyæmia.

Septicæmia was not unknown in Lister's wards, though it must have been rare by comparison with the wards in which I dressed.

When it did occur, it was held to be due to some defect in the management of the spray, or carelessness in changing the dressings.

By degrees it was found that chemical antiseptics were not the only factor to be considered in regard to the prevention of wound infection; in fact, disastrous experience soon showed that with regard to some wounds, such as abdominal sections, chemical antiseptics could not be employed. I need not trace the history of the evolution of antiseptic surgery into aseptic surgery, but with the development of the latter, and the lessons which had been learned in the process of evolution, the importance of the resisting powers of the body tissues against microbic infection again came into prominence.

You know how to-day chemical antiseptics have practically disappeared from wound treatment, how heat has taken their place as a means of sterilizing instruments and dressings, and how even the limited use of chemical antiseptics for hand disinfection is supplemented by the use of rubber gloves, while the omnipresent aerial germ of my student days is largely disregarded.

Every surgeon knows that the region of the anus, the scrotun and the vulva are amongst the most unsatisfactory for surgical operations, and by strict attention to securing the best conditions possible, by what Senn in a happy phrase termed pedantic asepticism, satisfactory results are obtained, but nothing like the brilliant results obtained in other parts of the body. It has always been a puzzle to me why an all-wise Providence should have chosen the immediate vicinity of the anus and urethra for the opening of the parturient canal, for surely this is about the last situation a modern surgeon would select if he wished to avoid septic contamination of a wound. No doubt it is quite true that, given an easy labour in a healthy woman with an intact fourchette and a vagina free from abrasion or contusion, the placental surface is fairly protected against any invasion of microbes; but let there be, as in a large proportion of cases there inevitably will be, some slight laceration of the vulvar orifice with some bruising and loss of tone of the vaginal walls, and what is easier than for septic infection to spread up the vagina and reach the body of the uterus?

There is a tendency now-a-days to regard all septic infection of the uterus as carried in by fingers or instruments, and in a leading article in the *Lancet* for August 7th, 1909, in reference to Professor Byers's address to the British Medical Association, the writer, speaking *ex cathedra,* says: “It is impossible to say that cases of auto-infection never occur, but as a rule in these cases a source of infection will be found either in some other part of the patient's body or in her environment.” Surely one need not look very far away from the opening of the parturient canal for a possible source of infection, given a sufficient amount of injury to the parturient canal to weaken its defensive or protective power?

I have already referred to the swing of the pendulum with regard to vaginal douching, and I cannot but feel that the present position is a very unsatisfactory one. It has been abolished, we are told, from well-conducted maternities, that is, as a routine; but it is still used in special cases, and that seems to be the custom with most of my medical friends. This leaves the question of necessity to the individual judgment in every case, and shows an absence of that finality and precision which are so desirable in a method of dealing with patients who must, in a large proportion of cases, be attended not by the most skilled medical men, but by an untrained woman, or, at the best, by women whose training is, after all, fairly rudimentary. That opinion is by no means unanimous even on the present prevailing custom in English practice is shown by the following extract from the epitome to the *British Medical Journal* for December 4th, 1909, the reference being to the routine technique in Pinard's wards at the Clinique Bandelocque:—"At the onset of labour a bath is given, and then follows subsequent cleansing of the external genitals and ano-perineal region with an iodol soap and a 1 in 4,000 solution of biniodide of mercury. Then a vaginal injection of the same solution is given and sterilized wool applied to the vulva. A vaginal injection is given after the delivery of the fœtus, and also after the expulsion of the placenta, then the external parts are washed, and wool re-applied to the genitals. This dressing is continued, and the parts washed night and day. The patent's linen is disinfected in Hersche's stove."

Now, personally, I think Pinard's technique excellent, and have no doubt that if it could be effectually carried out in the case of every woman, not only in hospital, but in the slums, on the lonely farms and in the native locations, it would still further diminish the mortality and morbidity from puerperal fever. But as a practical solution of the problem how to eliminate puerperal fever from our mortality returns, I fear it has no chance of success.

Before any further great advance can be made, it seems to me we must know more about the protecting and inhibiting powers of the body tissues, though I confess I have no suggestions to make as to how the knowledge is to be obtained. We all know what we mean by immunity, but in what does immunity really consist? We recognise that there is an original immunity, a natural immunity if you will, inherent in the body tissues—for instance, the immunity of all living tissues against putrefactive micro-organisms; we know that immunity is acquired against certain microbic diseases by previous attacks; that it can, at times, be artificially produced, as by inoculation with antitoxin for diphtheria; we believe it is often inherited. We know it is often developed by the patient during a disease. How often have we all opened an appendix abscess in removing an appendix, some peritoneal infection inevitably resulting, and yet the patient has recovered without a sign of trouble, or if for a short time there has been evidence of peritoneal infection it has rapidly disappeared—the patient having in the course of his illness before the operation developed his own antitoxins or opsonins, or in some other way become immune. On the other hand, who cannot remember other cases where everything seemed comparatively favourable, and yet a fatal result followed with appalling rapidity? Why had the one patient acquired an immunity that was denied to the other? Why, as I asked just now, does one patient die promptly from a mere scratch that 999 other patients would be hardly inconvenienced by?

And what about the other factor I have referred to—the potency or malignancy of the attacking microbe? Are we not too apt to look on all microbes of the same species as equal in their powers for harm? That is, do we not assign certain definite powers to the *staphylococcus albus*, the *staphylococcus aureus*, the *streptococci*, and so on, and expect more or less the same result from the attack in any case by the same individual microbe? And yet is that not most likely quite wrong? The powers of the microbe must and do vary enormously under circumstances which affect the microbe as well as under circumstances which affect the man.

Here we have a wide field for enquiry, or rather two fields—the varying resisting powers of the patient on the one hand, the varying attacking powers of the microbe on the other. When we can explain why, under the old conditions of midwifery practice which obtained when I was a student, such a large majority of women escaped, and why the small number succumbed, we shall have made a discovery which will go far to help us in getting rid of puerperal septic infection.

I have, I am sorry to say, no suggestions to offer. I have not dared to call my paper by any more ambitious title than "Some Thoughts on Sepsis," and I have no hopes of myself doing anything towards answering the questions I have raised. To quote once more from Tennyson, I feel myself to be, in these and many other matters,—

> "An infant crying for the light,
> And with no language but a cry."

Discussion.

Dr. Casalis, referring to the treatment of puerperal sepsis, stated that he considered it a mistake not to use the douche in such cases. He referred to the occurrence of acute pelvic fever in cases where no vaginal examination had been made.

Dr. Maxwell stated that the aseptic position in obstetrics was being slowly arrived at. He referred to the difference in the conditions in surgical and obstetrical practice, and quoted Macallister's statement, "Labour has become difficult in mankind," and referred to vaginal lacerations which are produced in normal labours.

He condemned the vaginal douching either before or after labour, and stated that efforts should be directed towards preventing germs from gaining admission to the genital tract, rather than to removing them after they have entered. He referred to the harmful effect of antiseptics on the mucous membrane of the vagina and uterus, and stated that douching failed to remove pathogenic germs, whilst it lessened the resisting power of the tissues.

The heavy mortality from puerperal sepsis in country districts in the Transvaal was thought to be due to dirty midwives, and was in marked contrast with the low death-rate in child-bed in the East End of London, where the conditions were far from ideal.

The question of early rising after labour was mentioned. Dr. Maxwell thought that this did good by emptying the vagina of discharges.

Dr. Rogers said we must ask ourselves the question why there is no diminution in the mortality from puerperal sepsis.

He stated that there were influences at work over which we had no control, and referred to three cases which had come to his knowledge of puerperal sepsis occurring in the same house, in the same or in adjacent rooms.

He referred to the blame which is often attached to the medical attendant in such cases, and to the need of instructing the public on the subject. He spoke of the use of antistreptococcic serum in puerperal sepsis, referring to a case in his own practice in which antistreptococcic serum was used successfully in an attack of sepsis following the first confinement: the patient died of sepsis after the second confinement.

He discussed the question of tubal infection, and the possibility of obtaining cultures from the vagina by means of which patients might be immunised against existing infection. He stated that he was in the habit of using rubber gloves at confinements.

Dr. Wessels spoke in favour of douching in leucorrhœal cases, or when a urethral discharge is present.

If infection occurred in small lacerations, he was in favour of local treatment rather than of douching. He spoke of the value of early rising after confinements, as thrombosis and subinvolution were less likely to occur in such cases.

Dr. Isaacs spoke of the danger to the public and annoyance to the profession which unqualified midwives in country districts gave rise to.

He mentioned three cases in which death resulted from sepsis in young women where unqualified midwives had been in attendance

III.—THE TRANSVERSE ABDOMINAL OR PFANNENSTIEL'S INCISION IN GYNÆCOLOGY.

By W. Gordon Grant, M.D. (Aberd.), Johannesburg.

During the past few years attempts have been made by pelvic surgeons in Europe to discover some incision which would give a firmer and more secure cicatrix than the longitudinal incision in the linea alba, without other disadvantages. Abel's statistics, published in 1895, showed that when the incision in the linea alba was closed by a single layer of sutures, hernia followed in 30 per cent., but when the different layers were united each to each this percentage was reduced to 8.

Langer has shown that the skin fibres in the lower part of the abdominal wall run nearly transversely, thus explaining why a longitudinal incision gapes and why, if the skin be cut transversely, the edges lie in close apposition. The continued tension on the edges of a vertical cicatrix gradually widens it, while in a transverse cicatrix there is no tension on the edges, and it retains its primary linear character.

Pfannenstiel published in 1900 details of his method of transverse incision, and recommended it, not alone for cosmetic reasons, but also as a more certain means of avoiding subsequent hernia. He advises an incision slightly curved upwards, crossing the linea alba about 6 cm. above the pubis, and about 6 or 8 cm. or more long. This incision is deepened through skin, subcutaneous tissue and the anterior layer of the recti sheaths. The fascial sheaths are separated from the muscle upwards and downwards by the fingers, and divided by knife or scissors from the firm attachment in the linea alba. A vertical incision is then made between the recti into the peritoneal cavity. In closing the wound the peritoneum, recti, fascia and skin are sutured separately.

If the incision be found not long enough, it can be prolonged into the transversalis and oblique muscles by cutting through the fascia and blunt separation of the muscle fibres which run almost in the line of the incision. Towards the outer ends of the incision the fascia will be seen to separate into layers, and care must be taken that these are all united in the sutures. The exact stoppage of all bleeding is essential to obviate the formation of a hæmatoma under the fascial layer, and, as a further precaution, some operators either fill the bladder immediately after the operation with some bland fluid or lay a light sandbag across the lower part of the abdominal wall. It is claimed for this method of incision that, in spite of the separation of the fascia from the recti, the nerves are never cut, and hence the chance of atrophy is non-existent; that the recti muscles, which are sutured together behind the fascia, form a strong muscular support to the cicatrix in the fascia; that there is no tension on the sutures in the fascia or in the skin, and hence the healing is practically ideal; that the cicatrix does not tend to stretch afterwards, but remains linear, and there is a better view of the field of operation, especially laterally. The recti give no trouble at the operation, and are easily pulled out of the way by retractors. There is no need for an abdominal belt, and a patient can get up very soon after operation, as there is no tension on the incision. In one of my cases where the incision broke down, there was very little separation of the skin edges, and a narrow scar resulted.

At first the indications were limited by Pfannenstiel to ovarian cysts of moderate size, non-suppurative diseases of the adnexa, small myomata, early ectopic pregnancies and ventrofixation of the uterus. These indications have been gradually widened, but most operators still prefer the vertical incision when there is danger of fouling the peritoneum in acute suppuration; when it is necessary to remove a large tumour complete, such as a multilocular cystoma or a solid or malignant tumour of the ovary; when very extensive adhesions are expected; and when the patients are very fat.

Since I began using the transverse incision I have opened the abdomen thirty-four times, in twenty-two cases using the transverse incision, and in twelve the vertical.

During the operation it is necessary to retract the recti, and this is best done by a self-holding retractor. Speaking generally, when the adhesions are very extensive, unless the incision be of considerable size, difficulty will be found if the incision be not lengthened. The operation through the transverse incision is not quite so easy as through the vertical. In two cases of extra-uterine pregnancy I adopted the vertical incision because the patients were suffering from active hæmorrhage, and I may here remark that it takes a longer time to open the abdomen by the transverse incision. In the cases of cystoma ovarii I used the transverse incision in two instances when the cysts were clearly free from adhesions, although they reached the metasternum. Two of the cases of myomata showed sarcomatous degeneration; this was not diagnosed before operation, but it was clear that operation would be very difficult. One case of pyosalpinx operated upon by the vertical incision was difficult, the tubes being of large size and very firmly adherent deep in Douglas's pouch. In two cases of Wertheim's operation the vertical incision was used, but I think that after a little more practice I shall choose the transverse in future. In a case of general peritonitis I used the vertical incision, because it was clear that there would be considerable fouling of the wound, and purulent material might get up along the recti sheaths, infecting them and causing trouble, and because of the necessity for drainage.

I am very well satisfied with the after results, both immediate and remote. In two of the early cases small hæmatomata formed, but these healed after the blood was allowed to escape. In one of the myoma cases considerable force had to be used to get the myoma through the wound, and there was a fair amount of bruising; the wound separated, but later healed, leaving a very narrow scar. Suppuration occurred in the subcutaneous tissue in a stout woman, but affected only one half of the wound; this also healed, leaving scarcely any scar.

I do not believe that this incision is applicable in every case, and at first one should observe Pfannenstiel's indications. One point I may add is that one may allow himself more latitude in a multipara where the abdominal walls are lax than in a nullipara with firm abdominal walls.

IV.—NOTES ON A CASE OF PROLAPSE OF THE FUNIS.

By Herbert Caiger, M.B. (Lond.), F.R.C.S. (Eng.),
Burghersdorp.

Prolapse of the cord is admitted to be a serious complication of labour. So far as I can recollect, I have seen three cases of prolapse of the cord in vertex presentations. Of the two earlier cases I have no notes. In both these cases the cord was replaced by the hand, and there was no evident recurrence of the prolapse; but the child in one case, although born alive, was so weak that it died a few hours after birth, and in the other case the result was equally disappointing.

In the third case, although the prolapse occurred early in labour, the child survived, and, with previous failures in mind, the successful result in this case appeared the more noteworthy. The patient was a multipara, aged thirty-five, who had had four children previously. The membranes ruptured spontaneously early in labour, while the pains were still slight, and there was a copious discharge of liquor amnii. The prolapse of the cord which occurred was fortunately recognised at once by the midwife in attendance, who realised the seriousness of the case, and sent for me. I found a considerable loop of the cord in the vagina. It was pulsating normally. The vertex was presenting, but the os only slightly dilated. The prolapsed loop of cord was replaced without difficulty by means of a gum-elastic catheter and a loop of thick sterilized silk. The catheter was passed up inside the uterus for nearly its whole length; the stylet was then withdrawn; but the catheter was left in place until after the delivery of the head.

Labour proceeded normally. No recurrence of the prolapse could be detected. Auscultation every few minutes during the next few hours showed that the fœtal heart was beating strongly and regularly. As a matter of precaution, however, forceps were sterilized and kept in readiness for immediate application if required.

About three hours after the reposition of the prolapse, the fœtal heart sounds, which had been fairly loud up till then, became rapidly weaker, and in a few minutes became almost inaudible. By this time the head was beginning to stretch the perineum. Forceps were at once applied, and delivery accomplished without difficulty. The child was much cyanosed, and did not breathe, but after fifteen minutes' artificial respiration it began to breathe regularly, and there was no further trouble. Both mother and child did perfectly well subsequently.

All three of my cases led to one very definite practical inference: that, however completely and satisfactorily the replacement of the prolapse appears to have been accomplished, there is still danger to the child. In none of my cases could any recurrence of the prolapse be detected, yet in all of them something went wrong, making the child's condition a critical one, or worse. What was it that happened? Possibly a loop of the cord got so low down as to be caught between the child's head and the pelvis as the head descended, but yet did not come low enough to be detected by the examining finger. Possibly the descent and replacement of the prolapsed loop somehow altered the relations of the cord in such a way as to render it more liable to become kinked or knotted when drawn upon by the descent of the fœtus. But whatever be the explanation, the fact remains that replacement of the cord does not put an end to the danger to the child, and the practitioner must be on the alert, and ready to hasten delivery on the first sign of danger.

Perhaps it may have been through not recognising this that I lost my first two cases. But, until taught otherwise by experience, it was natural to assume that, having replaced the prolapse, and there being no apparent recurrence, all danger was over. The text-books I have consulted might, I think, emphasise this point more clearly; it is perhaps implied in some of them, and watchfulness is enjoined, but seeing that the feeling of security is but natural when the prolapse has been replaced, the counsel of watchfulness needs to be all the more emphatically given.

Another very definite impression from my last case was the value of frequently repeated auscultation as a guide to the condition of the child, and the very definite character of the warning given. Had I used this method in my earlier cases, it is possible the result might have been different. In the third case, the fœtal heart sounds were loud and regular; auscultation was performed every few minutes, and for three hours there was no change; then, as the head of the child came down on to the perineum, in the course of a few minutes the fœtal heart sounds diminished so rapidly in strength as to be almost inaudible. There could be no mistaking the urgency of the warning so given, and forceps were applied at once. The asphyxiated condition of the child at birth showed that any longer delay might have been fatal.

As regards the catheter and loop method of replacing the prolapse the idea of leaving the catheter in place in the uterus struck me as being a good one. It is recommended by Dr. Tate, of St. Thomas's Hospital, in his article on the subject in the "Encyclopædia Medica," which I consulted at the time, and later, on consulting Galabin's book, I found that he, too, advises it. The method I adopted was as follows: A large gum-elastic catheter was used, and a small hole cut opposite the eye of the catheter; a piece of thick sterilised silk was passed through these holes and tied so as to form a loop, which was passed round the prolapsed cord, and then hitched over the end of the catheter. The catheter with the stylet in it was then passed up inside the uterus, almost for its whole length. The stylet was then withdrawn, but the catheter left *in situ* till the head was born. If properly sterilised, the retention of the catheter and loop in place in the uterus can do no harm, and it certainly would seem to give greater security against a recurrence of the prolapse.

The following practical inferences which I wish to submit for your consideration may be shortly summed up as follows:—

(1) All danger is not over when the prolapsed cord has been satisfactorily replaced, and no false sense of security must be entertained. There may be no recurrence detected, but none the less disaster may follow at a later stage from some interference with the circulation in the cord, the mechanism of which is not clear.

(2) It is therefore necessary up to the moment of birth to test the condition of the child by auscultation repeated every few minutes. In cases of vertex presentation forceps should be got ready for immediate application, and if any marked weakening or slowing of the fœtal heart sounds be noted, delivery should be accomplished as speedily as possible.

(3) When the prolapse is replaced by means of catheter and loop, it is advisable to leave them *in situ* after removing the stylet; there is then less risk of a recurrence of the prolapse.

Discussion.

Dr. Howell Davies said that this was a rare but unfortunate complication. The suggestion as to retaining the catheter *in situ* was a good one. He related a case in which he was not provided with a catheter. He replaced the cord, but it quickly came down again. He then put the patient in the genu-pectoral position and delivered, but, although the compression was of very short duration, the child died.

Dr. Wessels said that the position of the cord was important: if in front of either sacro-iliac, there was less chance of complete compression; likewise if the sections of the loop were on different sides of the presentation. It was often better to do version and not waste time. These cases generally occurred when there was much liquor amnii. The genu-pectoral position was a help, as was also some elevation of the pelvis.

Dr. Simpson Wells said this complication sometimes occurred when there was hydramnios, and in such cases the vitality of the child was low. Nagel thought that it was a good practice, in certain cases, to leave the cord down, or even draw it down a little lower. Replacement of the cord was often extremely difficult. The statistics of the results of version were not favourable, and some cases, according to leading German authorities, do well left alone and carefully watched till the time comes for application of forceps.

Dr. W. H. Maxwell said that obstetrical works treated this subject scantily. The typical causative condition was an imperfect fitting of the head into the pelvis. Flat pelves, which were not met with in this country, or very rarely, were very frequently the accom-

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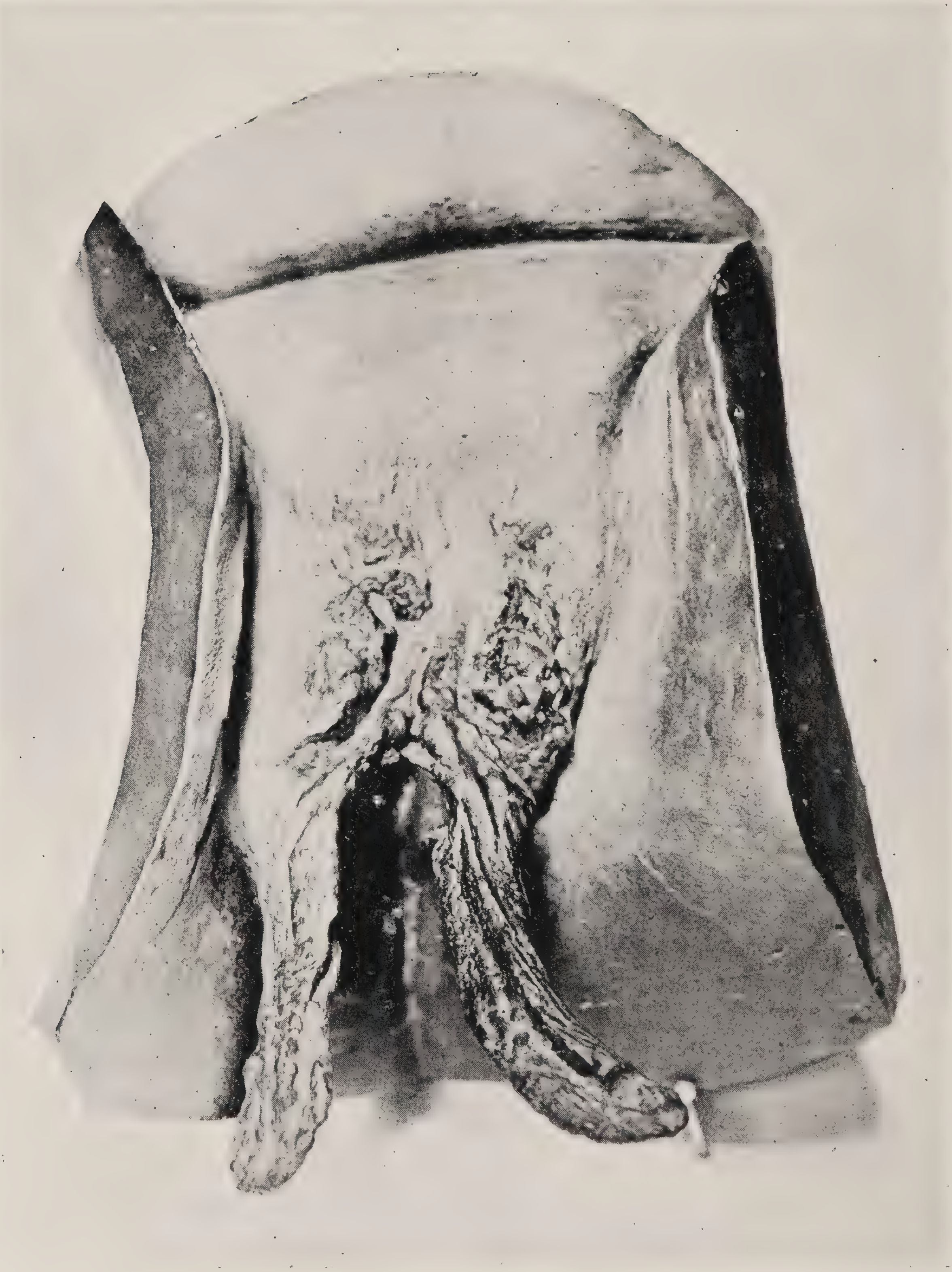

Photo of an actual Cast taken by Mr. DRURY (Modeller to the Museum) from an unmarried Hottentot, aged about 40 years.

Cast lent by Dr. PERINGUEY, Director, South African Museum, Cape Town.

Labium 3¾ in.

paniments of prolapse. It was a good idea to push the cord up, apply forceps, and pull the head down so as to fit the pelvis. There was certainly no harm in leaving the catheter *in situ*. With prolapse recurring, replacement was most difficult.

DR. PORTER MATHEW had had thirty-six cases, and he had followed the advice given by Dr. Maxwell.

DR. CAIGER said that his first case was apparently caused by premature rupture of membranes and sudden rush of the liquor amnii, although there was no actual hydramnios. The danger was that the replaced cord might again come down sufficiently far to be compressed, and yet not sufficiently far to be reached by the examining finger. The noticeable point was the rapidity of the change in the fœtal heart, occurring in a few minutes after having been regular for three hours, and unless he had been listening every few minutes this would not have been detected in time.

V.—THE NYMPHÆ.

(*Anthropological.*)

BY A. MARIUS WILSON, M.D., F.R.S. (S.Af.), CAPE TOWN.

The nymphæ, or labia minora, are two folds of mucous membrane, highly sensitive, and entirely concealed by the labia majora in early life. They contain many sebaceous follicles, more numerous on the internal surface, which secrete cheesy matter with a peculiar odour; and, besides connective tissue, numerous blood-vessels, and nerve endings, show some non-striated muscular fibres which give them erectile properties. The anterior portion of the nymphæ converge and bifurcate, the upper portion forming the prepuce of the clitoris, and the lower, attached to the clitoris, forming the frænum. In early life the nymphæ are soft, moist, and pink in colour; in later life, when the labia majora shrivel up or atrophy, the nymphæ project, dry up, become more like the surrounding integument, and change in colour from pink to dark-brown. In some instances the nymphæ hypertrophy—masturbation being usually credited therefor—and in certain races it would appear that elongation of the labia minora is common, and is spoken of as "the apron" or "lapel." This elongation is supposed by some to be a special mark of beauty, and, if this be correct, special steps will surely have been taken to encourage and increase this enlargement.

Whatever takes place to-day amongst European peoples, in earlier times chastity in females was enforced occasionally by means of cages or padlocks fixed on to the external genitals. In some cases this infibulation was performed by stitching together the two labia minora, leaving only a tiny aperture to allow of the escape of menstrual fluid. This appears to be a custom, a religious rite, amongst certain races. In the Eastern Sudan, Dr. Anderson, describing "Circumcision of Girls," states that the operation is done at the age of five or six, and is attended with much less ceremony than in the case of boys. The girl is placed lying down on a bed, and kept in this position by assistants, who also keep the legs apart. The operator, usually an old woman, severs the clitoris and the two nymphæ with a sharp instrument; bleeding is arrested by bathing with hot water; a special powder is applied, and the legs are then kept bound together for one week. The raw parts adhere, and the vaginal orifice is closed, save for a very small opening to allow menstrual fluid to escape. On the wedding day the bride undergoes a further operation—this time to open up the vaginal orifice. This is done again by an old woman, who again uses a sharp knife, which knife, covered with blood, must be shown to the wedding guests. At this ceremony the husband is present, armed with a sharp instrument, and is entitled to cut away further to his satisfaction. Dr. Anderson closes his description of these operations by quoting an Arab saying: "Woman is a leathern water-bottle; we sew her up, open her, and fill her up as we please."

A recent publication on a prehistoric race in British East Africa shows that operations on the nymphæ are not confined to any one race in Africa. This book states that the Akikuyu girls are operated upon between the ages of ten and fifteen years, that the nymphæ and clitoris are removed, and that medicine is given to relieve the pain of the operation. It appears also that boys and girls are operated upon in separate batches, and with much preliminary ceremonial. After the operation is over, the candidate is no longer a boy or girl, but a man or woman.

The rites above mentioned do not appear to be practised amongst the indigenous races of Southern Africa. On the contrary, the nymphæ are here enlarged, and it is the object of this paper to try to find out if this enlargement is a natural or forced growth. As Professor Balfour in his recent lectures on Anthropology pointed out, every year makes it more difficult to answer many anthropological questions, and there must be an enormous amount of scattered knowledge which could be collected and made very considerable use of. Our knowledge of these enlarged nymphæ is about what it was in 1801, when Barrow's account was published. This is what he says:—

". . . An extraordinary character that distinguishes the other sex from the women of most nations. The well-known story of the Hottentot woman possessing an unusual appendage to those parts that are seldom exposed to view, which belonged not to the sex in general, is perfectly true with regard to the Bosjesmans. The horde we had met with possessed it to a woman; and, without the least offence to modesty, there was no difficulty in satisfying curiosity. It appeared on examination to be an elongation of the nymphæ, or interior labia, more or less extended according to the age or habit of the person. In infancy it is just apparent, and in a general way may be said to increase in length with age. The longest that was measured somewhat exceeded five inches, which was in a subject of middle age. Many were said to have them much longer. These protruded nymphæ, collapsed and pendent, appear at first view to belong to the other sex. Their colour is that of livid blue, inclining to a reddish tint, not unlike the excrescence on the beak of a turkey, which, indeed, may serve to convey a tolerably good idea of the whole appearance both as to colour, shape and size. The interior lips or nymphæ in European subjects, which are corrugated or plaited, lose entirely that part of their character when brought out in the Hottentot, and become perfectly smooth. Though in the latter state they may possess none

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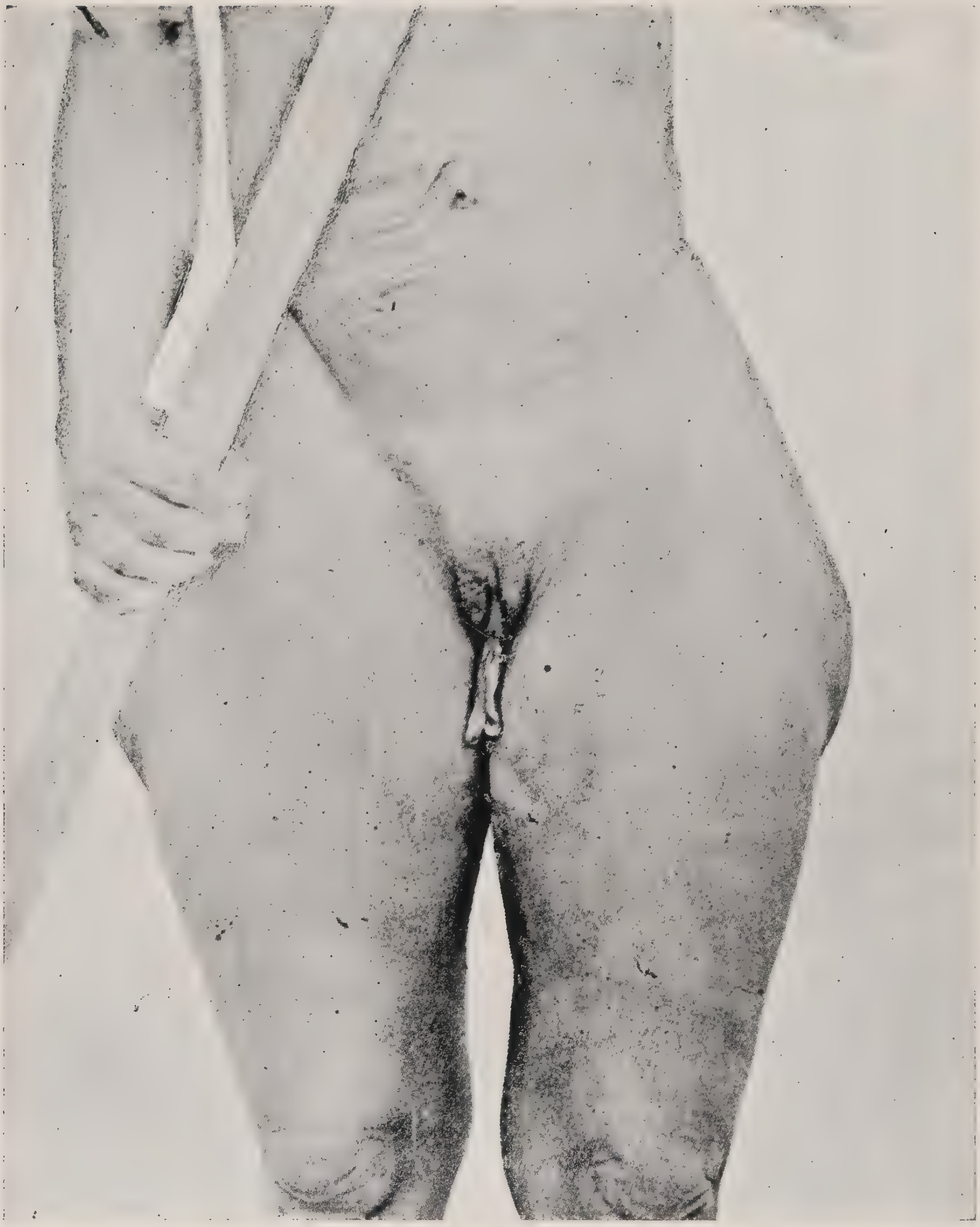
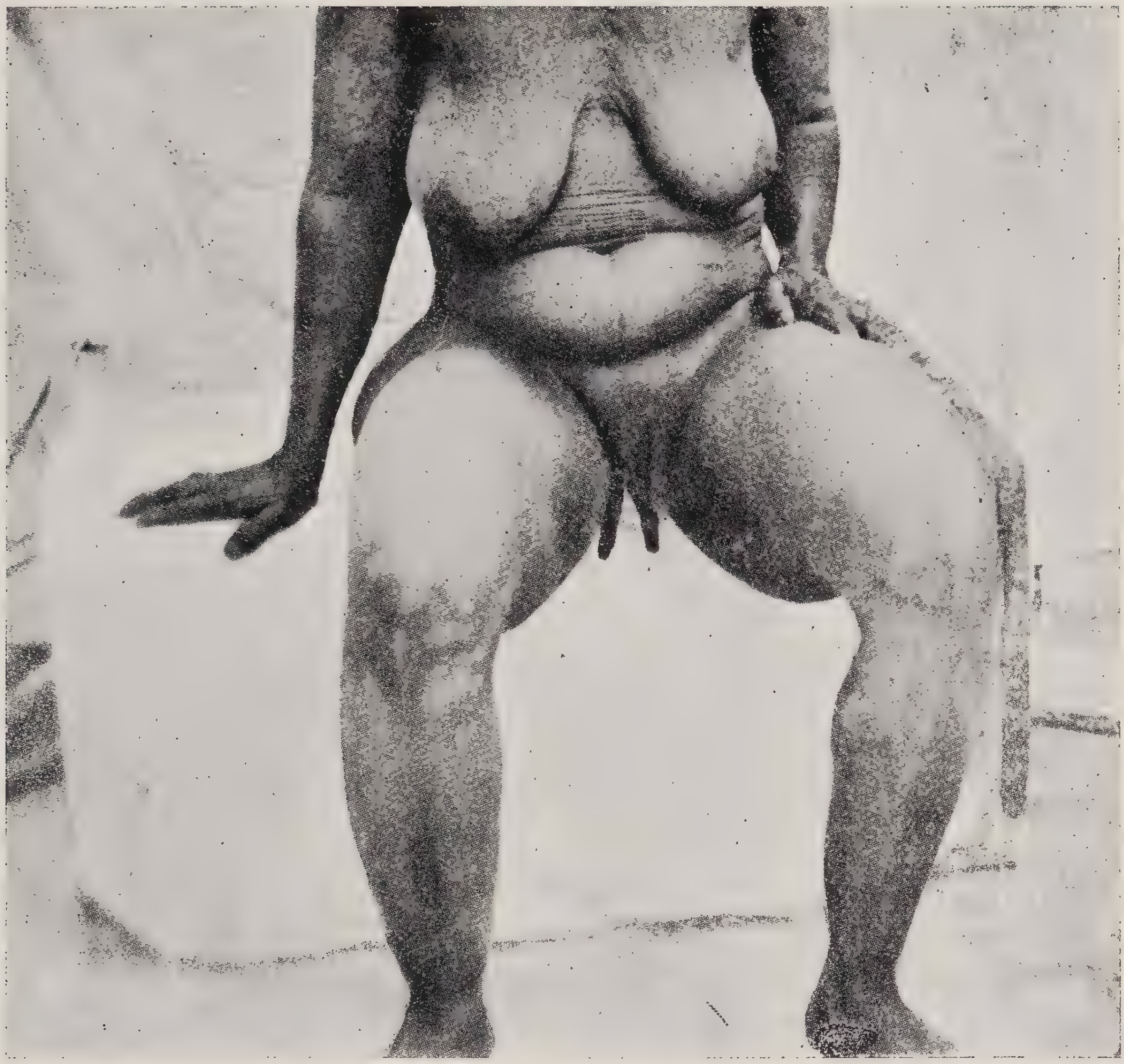


Photographed from life.

Cast in S.A. Museum.



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Cast in S.A. Museum.

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of those stimulating qualities for which some anatomists have supposed Nature to have formed them, they have at least the advantage of serving as a protection against violence from the other sex, it seeming next to impossible for a man to cohabit with one of these women without her consent, or even assistance.

"Nature seems to have studied how to make this pigmy race disgusting; though a certain French traveller has thought fit to exculpate Nature on this point, by asserting the above-mentioned conformation to be entirely the effect of art. The testimony of the people themselves, who have no other idea but that the whole human race is so formed, is sufficient to contradict such a supposition; but many other proofs may be adduced to show that the assertion is without any foundation in truth. Numbers of Bosjemans women are now in the Colony who were taken from their mothers when infants and brought up by the farmers, who, from the day of their captivity, have never had any intercourse whatever with their countrymen, nor knew, except from report, to what tribe or nation they belong, yet all these have the same conformation of the parts naturally, and without any forced means. The story of their prepending pieces of stone in order to draw down the interior labia is still popular in Bruyntjes Hoogté, where the author above alluded to received it. . . . .

"The elongated nymphæ are found in all Hottentot women, only they are shorter in those of the Colony, seldom exceeding three inches, and in many subjects appearing only as a projecting orifice or elliptical tube of an inch, or less, in length. In the *bastaard* it ceases to appear —a proof that a connection with different nations counteracts the predisposition to such a conformation.

"It is not, however, to the southern angle of Africa alone that the same predisposition for the elongation of the nymphæ is confined. The physical causes that tend to the production of so extraordinary an effect operated in parts of Egypt, situated under the same and opposite parallels of latitude as the Hottentot country. It was here, however, considered as a disease, an appearance so deformed and disgusting that those who were troubled with it were glad to undergo the violent pain of the actual cautery in order to get rid of it."

Regarding this last paragraph, it would be interesting to learn from Dr. Anderson, in his next report, whether the girls operated upon for infibulation show any signs of elongation of the labia minora; or if the operation owes its origin in any way to the idea of getting rid of a disease, which Barrow states was what the elongation—a disgusting deformity—was thought to be. Kolben, who described what he saw and heard in 1705, also refers to elongated nymphæ occurring amongst other than Hottentot women.

"The Hottentot women have all a remarkable excrescence, the description of which must be here given. It is a kind of skin, hard and broad, which grows above the os pubis, and which, hanging somewhat low, seems intended by Nature to hide the woman's nudity. Over that, however, they wear a piece of sheepskin, called *kut-krosse*. This excrescence is, in some individuals, so long that it cannot be entirely hidden by the skin that should cover it. This deformity—as it must appear to European women—this natural skin is not looked upon as a blemish by the Hottentot women or their husbands. If dirt and grease do not act as a deterrent for examining closely and handling this excrescence, your curiosity can be satisfied for a little tobacco or other trifle."

"*Thevenot* says that the Negro women and the Egyptians have also the same skin, but these women do not allow it to grow; they excise early, or, rather, they cauterize it with a hot iron."

Cuvier's remarks, in connection with the well-known "Hottentot Venus" may now be given. He says:—

"She died on December 29th, 1815. The body was brought to the Museum, and it was carefully examined. The first object was to examine the extraordinary appendage with which it is said that Nature has endowed this race. It was easily found, and, although having to admit that it was exactly the reproduction of Perron's sketch, it was not possible to adopt the theory formulated by this indefatigable naturalist. In effect the 'apron' is not, as he stated, a particular organ; it is a development of the nymphæ. The 'labia majora,' little pronounced, intercepted an oval 4 inches in length; from the upper angle there descended between the labia majora a semi-cylindrical prominence, about 18 lines long and 6 lines thick, the apical extremity of which thickens, divides, and is continued in the shape of two fleshy wrinkled petals 2½ inches in length and about 1 inch broad. Each of these appendages (petals) is rounded at the end; their base widens, and goes down along the inner border of the labium major on its side, and turns into a fleshy crest ending at the inferior angle of the labium.

"If these appendages are raised, they make together a heart-like figure, the lobes of which are narrow and long, and the centre of which would be occupied by the opening of the vulva. On close examination it will be seen that each of these two lobes has at its anterior face, and very near the inner border, a furrow more distinct than the other wrinkles, and which ascends above the bifurcation; there the two furrows unite, so that there is at the bifurcation a double fold surrounding a chevron-like fossa (depression). In the centre of that depression is a slender prominence ending in a small projection where the two inner folds unite.

"It is manifest to whoever reads this description, and better still to him who will compare these parts with analogous ones in European women, that the two fleshy lobes that form the apron consist in the upper part of the prepuce and of the summit of the nymphæ, and that all the remainder of the length is merely a development of the nymphæ alone. The internal parts of the vulva and of the womb had no peculiarity. . . .

"As to the idea that these excrescences are artificial, it would seem to be disproved if it is true that all Bushwomen have them from their youth. The woman whom we saw had probably taken no pleasure in acquiring an ornament of which she was ashamed, and which she hid with so much care. The apron (veil) of the Bushwoman is not one of those peculiarities that might prove a connection between women and monkeys, because, far from their being long in the latter, they are generally barely visible."

Most of the older writers seem to have been quite satisfied that the long elongated labia minora found amongst the Hottentot and Bushwomen were natural growths, but they make no statement as to when these elongations first appear—birth, puberty or later. The question of artificial aid is referred to only to be brushed aside. Spaarman, however (1775), evidently thought that the growth was not altogether natural, for he says:—

> "It is probably due to a relaxing effect necessarily brought about by the custom in which they indulge of smearing the body, by their lazy ways (inaction), and by the heat of the climate."

From his writings also, it would appear that he believed this elongation of labia occurred in "those who are no longer young."

The main object of this paper is to bring the matter before our medical confrères, especially to those whose work lies wholly, or in part, in those districts where these Hottentot women live in numbers, and who may be able to collect sufficient evidence to settle the questions which arise out of the consideration of a paper such as this. The writer would thankfully receive any authentic information which would throw light on the subject, which must be of interest to all who are anthropologically inclined.

To continue. If this elongation is produced, and is not natural, how far back does the custom go, and is it a ceremonial rite or not? The suggestion has been made that the elongation is looked upon as a mark of beauty, though Cuvier says that the "Hottentot Venus" thought very differently. But was this only after she had left her own country? Barrow states that "they have at least the advantage of serving as a protection against violence from the other sex," and, if this is so, one can understand that the possession of serviceable enlarged nymphæ may have been very valuable.

The labia minora posteriorly combine to form the fourchette, and this appears to be wanting in the Hottentot woman. It may be that the native midwife considers it her duty to break down the fourchette of every new-born baby girl, and thus start the formation of the "apron." Most of us have met in highly-civilised countries the kind lady who, despite all our reasonings, threats, or appeals, will "break down the nipple-strings" of the newly-born girl, and who has either sorrow or contempt (or both) for—from her point of view—our ignorance on such a very vital subject.

That the enlarged nymphæ is not at any rate entirely natural is mentioined by a recent writer, Dr. M. Hewat. He tells us that when the native girl commences menstruating, she is sent to a special native school, "and turned in what is known as a 'Bale.'" Definite information regarding the ceremonies are wanting, but it seems that there is no removal of any part of the anatomy as in circumcision.

> "The result of the treatment is that the labia minora are considerably elongated, sometimes very much so, and the female thus converted into an animal of lust and desire."

Let us hope that more definite information will soon be forthcoming.

VI.—UTERINE CANCER COMPLICATING PREGNANCY.

By D. H. Wessels, M.B., Ch.B., F.R.C.S. (Edin.).

Cancer of the uterus as met with during pregnancy almost invariably affects the cervix. It is a rare condition, since, if it is far advanced, it usually prevents the occurrence of conception. Winkel recorded eight cases in 15,000 labours, Sutuyin two in 9,000, and Stratz twelve in 17,000; in other words, taking all these cases together, a percentage of 0.047.

During my period as Assistant Master at the Coombe Hospital, Dublin, not a single case was met with in 10,000 labours.

Course of Pregnancy.—In some 15 per cent. of cases the pregnancies are interrupted, and in the remainder at least a third of the children are still-born, even at term, the proportion being much larger in cases of premature delivery (Edgar).

If the fœtus cannot pass the obstruction, a delayed labour results, and cases are on record in which the women thus afflicted have been in labour for over a week (Edgar).

Under these circumstances maternal death from exhaustion, or death and putrefaction of the fœtus, or general septicæmia, may occur.

Diagnosis.—The recognition of cancer of the cervix should not be difficult. Unfortunately cases are often not examined when symptoms first make their appearance. In the case recorded here, the patient was treated for some months for threatened miscarriage before an examination was made. Cases are also recorded where a fungating cancer of the cervix was diagnosed as placenta prævia. The rarity of the association of uterine carcinoma and pregnancy induces me to record a case which came under my observation in July, 1908. The case also shows the importance of an early examination in all cases of uterine hæmorrhage, whether associated with pregnancy or not:—

Mrs. D., aged 40, married seventeen years. Six children, youngest two years. No abortions. Previous health very good, except for a slight leucorrhœal discharge since puberty. Previous labours were normal, and there had been no puerperal complications, except "white swelling" of the leg after the first.

Family History.—An aunt died from cancer of the breast.

Present History.—Some months before she became pregnant there was a slight increase in the leucorrhœa. Her last period was seen on October 1st, 1907. Six weeks later the discharge became bloodstained, and soon slight hæmorrhage occurred at intervals of a week or ten days. There were no pains. In April, 1908, when she was six months pregnant, she noticed pure blood trickling away from her on going to the lavatory one day. Since then the hæmorrhages have been more profuse and more frequent. She then consulted her doctor, who thought she was threatening to miscarry, and she was advised to keep quiet. As no improvement followed, she was put to bed for some weeks. The hæmorrhage, however, gradually got worse, and early in June she was examined for the first time, when a growth was discovered on the cervix. A section of this was removed, and on microscopical examination proved to be an adeno-carcinoma. She was advised to seek immediate surgical aid, but it was not before the middle of July, 1908, and about the time labour was expected, that she was admitted into the Monte Rosa Hospital for operation.

State on Admission.—The patient, a well-developed woman, was very anæmic and sallow of complexion. She reported some loss of flesh, but no pain to speak of. The membranes had ruptured spontaneously the night before admission into hospital, and all the liquor amnii had escaped. No pains, however, came on, and there were no signs of labour setting in before the operation two days later.

On examination, the cervix was expanded into a fungating mass, almost the size of a closed fist. The growth involved three-quarters of the circumference of the cervix, and extended posteriorly into the right fornix and about a quarter of the way down the vaginal wall on the same side. The growth bled freely as a result of the examination, and the discharge also was very offensive.

Operation.—Vaginal Cæsarean section, followed by vaginal hysterectomy, was thought to offer the best chance for the patient, on account of the advanced state of the growth and the weak condition of the patient. I was assisted by Dr. Rowan, and the anæsthetic was administered by Dr. Alston. The operation took three-quarters of an hour. After cutting and scraping away the bulk of the fungating mass, the cervix was drawn down by means of stout silk ligatures. A transverse incision was made through the anterior vaginal wall above the margin of the growth, and the bladder stripped off the cervix. This was done very readily, even more so than in cases of ordinary vaginal hysterectomy. The cervix was caught laterally with stout Museaux forceps and split with scissors in the middle line anteriorly. By strong traction and by grasping the cut edges higher and higher on either side the splitting was continued up to the reflexion of peritoneum, without opening the peritoneal cavity. The child presented in the left occipito-anterior position.

Internal version was performed, and the child delivered as a breech. Delivery was accomplished fairly rapidly. The child, a well-developed female, weighing 6½ lbs., was a little asphyxiated, but was soon restored with artificial respiration by Dr. Alston.

Immediately after delivery of the child, the chord was clamped and divided, and the placenta removed manually. Bleeding was smart at this stage, but stopped with traction on the cervix, which was grasped again and pulled down with forceps. The peritoneal cavity was then opened, and the splitting continued through the uterine wall until the uterus was divided into two halves and delivered outside the vagina. Clamps were now placed on the broad ligaments, and the two halves of the uterus removed. Some difficulty was experienced in doing this on the right side, on account of the rigidity and thickness of the broad ligaments on that side. It was found impossible to remove the growth entirely. Careful plugging with iodoform gauze into the pelvis and between the clamps stopped any further oozing, and the patient was put back to bed. She showed signs of collapse, but was soon restored by slow, continuous saline injections per rectum.

The clamps were removed after forty eight hours, and the gauze packing gradually removed after the fourth day. The patient made a good recovery, and left hospital eighteen days after the operation. She rapidly gained in health, and was able to attend to her duties, and even to play croquet, for fifteen months, until extension of the growth in the pelvis began to worry her again. She died nineteen months after operation. The child lived for six months, but unfortunately died from summer diarrhœa.

In the treatment, in general, in cases of carcinoma of the cervix complicating pregnancy, the question at once arises: Should one operate in *early* pregnancies immediately the complication is recognised?

Schauta mentions a case in which a woman, five months pregnant, had a carcinoma of the cervix limited to the anterior lip. The carcinoma was excised and the diagnosis confirmed. Normal delivery occurred at term, a healthy male child being born. Operation for the recurrence took place three years later, and the patient was quite well twenty years later. He states that he would not stretch a point in favour of excision of carcinoma in pregnancy, but adds: "One ought not, *without exception*, to sacrifice the whole uterus in a case of early carcinoma in pregnancy."

The choice of operation is determined by the state of the carcinoma and by the condition of the patient. *Up to the fifth month* vaginal or abdominal hysterectomy may be performed. Most operators prefer the vaginal route, as in pregnancy the uterus is plastic, and it is possible to reach with ease far out into the broad ligaments, a feat much more difficult in the case of the non-pregnant uterus. (Kelly.)

In the later months of pregnancy, Cæsarean section, with the entire removal of the uterus, has been the usual procedure, but for some years past the favourite method has been vaginal Cæsarean section, followed by vaginal hysterectomy, as recommended by Durrhsen, Olshausen, Czerny, and others. More recently the Cæsarean Wertheim operation has been adopted, with good result, but up to last year only fifteen cases have been recorded, two by British and thirteen by Continental operators. Greater security from recurrence is promised by this method than by any other, but the primary mortality is greater.

In inoperable cases, Cæsarean section at or near term offers the best chance for the child and for the mother; it is better to continue the operation by amputating the uterus at the cervix, in other words by performing the Porro-Cæsarean operation (Kelly).

DISCUSSION.

DR. J. P. GRANT stated that his partner, Dr. W. D. Miller, had attended the patient whose case was reported in the paper. Dr. Miller saw the patient in the morning, and he was called to see her in consultation the same day, so that the delay referred to had not taken place whilst the patient was under medical care.

MAJOR KNAPP said that he had also seen the patient later on, and that the question had been raised as to whether the growth was not adenomatous. On microscopic examination carried out by Dr. Robertson, the malignant character of the growth had been established.

DR CAIGER spoke of the rapidly fatal course which such cases often followed. He referred to a case in his own practice in which miscarriage and septicæmia had resulted in a fatal issue.

DR. MAXWELL discussed the question as to whether the life of the mother or the child should be considered of more importance. In Britain the life of the mother was considered of paramount importance, but on the Continent efforts were usually directed to save the life of the child.

Although hysterectomy was not a pleasant operation when pregnancy existed, it should be done as soon as ever the tumour was detected, no matter what the stage of pregnancy.

Dr. Porter Mathew raised the question as to why the vaginal route was chosen in this case.

Dr. Wessels, in reply, stated that he thought the vaginal route was preferable because of the septic mass in the cervix. He had used clamps rather than ligatures, as he preferred them in such cases because they shut off the lymphatics more effectively, and drainage was better.

With regard to the history of the case, he had relied on the statements of the patient, and had not had an opportunity of communicating with Dr. Miller, under whose care she had originally been, and he regretted the inaccuracy.

VII.—OBSTETRICAL JOTTINGS.

By J. E. Waterston, M.D. (Brux.), M.R.C.P. (Irel.), Cape Town.

(Not supplied for publication.)

E.—EYE, EAR, NOSE AND THROAT SECTION.

I.—DIAGNOSIS AND TREATMENT OF HEADACHE DUE TO DISEASES OR DISTURBANCES OF EYE, EAR, THROAT OR NOSE.

By A. Greene Brinton, F.R.C.S. (Edin.), L.R.C.P. (Lond.),
Johannesburg.

(*Special Subject for Discussion: First Paper.*)

I propose to deal with headaches arising from diseases of the eye alone, and hope to bring before you conclusive proofs that headaches, particularly the chronic forms, are in a very large proportion of cases due to astigmatism of very low degree. I am taking this opportunity of placing on record the results of eight years' work in this direction. The figures are not taken from hospital statistics, but from my own case books of private practice; I have been unable to find any figures published which in any way approach my line of research, and though Dr. Gould of Philadelphia and one or two others have called particular attention to the causes of headache, it is surprising how conservative some practitioners are in failing to realise the important part that astigmatism plays in the daily life of half of their patients.

It is fairly safe to say that headache is a sensation or a symptom of which every civilized being at some time or other has been cognizant, in fact probably it is the more felt because of civilization, as is so often borne out in the case of natives when they become civilized invariably suffering more than their brothers. It is this commonness of headaches and of their recurrence that often makes the patient afraid to reiterate his complaint when he meets his doctor, whereas if the practitioner is convinced by the results of our discussion, he will in future tackle a headache on the right lines with confidence, taking no notice of the patient's statements of good vision or that he is a "first-class rifle shot." It is the procrastination caused by aspirin, phenacetin and aperients which causes the loss of patience and sometimes of patients also; therefore I say it is greatly to the doctor's detriment not to recognise the prevalent causes of this recurrent ailment. These causes are anything which directly or indirectly—by stimulating or irritating the sensory nervous system of the cranium—causes a vasomotor stimulation of the sympathetic.

In all parts of the body we recognise that pain is caused by pressure on the nerve endings, which pressure is caused by congestion consequent on the dilatation of the blood-vessels; a kick on the shin, as most people are aware, causes pain, but do they recognise that the pain is only the manifestation of pressure from vascular dilatation following the stimulation of the vasomotor fibres of the sympathetic?

In the same way a blow on the head may cause localised headaches at the seat of the blow from the subsequent immediate congestion, or it may cause a general headache through the slight concussion within the cranium, being the exciting cause of vasomotor stimulation and consequent flooding of the capillaries.

I will now deal with the causes of headache which come more especially under my notice—that is, peripheral irritation of the trigeminus in all its many branches, any one, two or more of which may be the source of the trouble at one and the same time; therefore I shall venture to remind you of the anatomy of the sensory portion of this important nerve.

You will remember that this portion forms firstly the Gasserian ganglion. This ganglion receives filaments within the cranium from the carotid plexus of the sympathetic, and gives branches to the tentorium cerebelli and dura mater; it then sends three large branches forward. The first or ophthalmic branch supplies the eyeball, lachrymal gland, mucous lining of the eye and nasal fossæ, besides the skin of brow and nose. This branch receives filaments from the cavernous plexus of the sympathetic, and also communicates with the third, fourth, and sixth nerves, and it is very important to note for the purposes of this discussion that, apart from its connection with the sympathetic, it gives off recurrent filaments which pass between the layers of the tentorium, and that its distribution is the nasal mucous membrane and the conjunctiva; and more particularly that the nasal branch gives off two or three ciliary nerves, which, together with the short ciliaries from the ciliary ganglion, are distributed to the ciliary muscle, iris and cornea. The second great trunk arising from the Gasserian ganglion is the superior maxillary, and its branches which are of immediate interest to us are, firstly, the meningeal supplying the dura mater, the spheno-palatine to the ganglion of that name, the labial branches to integument of upper lip and mucous membrane of the mouth, and, lastly, the numerous branches to the teeth of the upper jaw. The third great trunk is the inferior maxillary—this again sends a filament to the dura mater; it then only interests us on account of its supplying sensation to the buccal mucous membrane and teeth and gums of lower jaw, and its communication with the ear through the otic ganglion.

I should like here to remind you of the functions of the ganglia I have mentioned, *viz.*, the ciliary, the spheno-palatine and the otic. These ganglia naturally receive sympathetic, sensory and motor fibres, and are, therefore, in constant communication with the cranium *via* the carotid and cavernous plexuses. The former ganglion is of importance to us because of its branches to the uveal tract and cornea; the spheno-palatine because of its wide distribution over the naso-pharnyx and palate; and the otic through its connection with the internal ear.

From this *résumé* of the distribution of this, the largest sensory nerve of the body, you perceive how intimately related the eyes, ear, nose and throat are to one another and to the large vascular system of the membranes within the cranium, so that any irritation within any of these organs may cause a corresponding vasomotor stimulation *via* any of these direct routes.

Having shown you what, in my opinion, a headache is, and through what channels it is caused, I will ask you to consider the following figures. During the eight years I have been in this country I have kept notes of practically all refractions which I have done in private, namely, 2,500. In 838 patients the chief reason for coming or being sent to me was "headache," and the word was noted at the time. These were not cases where the patients sometimes had a headache, as most of us do, but where the headaches were so constant that they were a source of trouble to them, and in nearly all cases were the sole cause of their coming to me. It would have been easy to increase this number by asking the patient, "Do you ever have a headache?" This number is simply compiled of those whose habitual plaint was "headache." Twelve of these 838 had no error of refraction, 17 had an error of pure hypermetropia or myopia without astigmatism, and 809 had astigmatism. Of the latter 120 had astigmatism of 1 D. or over; 75 had one eye astigmatism of 1 D. or over and the other of under 1 D., and, finally, 614 had astigmatism of less than 1 D.; the latter is, to my mind, the important figure.

In these figures I have valued the astigmatism only, though the spherical correction must have a certain amount of influence on the condition, but seventeen headaches from spherical errors alone is so small a number that without the superadded astigmatism there is, in my opinion, very little influence by a regular error of hypermetropia or myopia. I therefore emphatically lay it down that the most prevalent cause of headache is astigmatism of under 1 dioptre. I do not say that there were not other causes at work in some of these patients to create headache, such as some nasal or other irritation of the fifth nerve, but a very small proportion of them came back for further treatment, and a certain number with other obvious ailments were recommended to have the services of other medical men. But I usually find that other treatments have been tried previously to the patient coming to my rooms.

I have not had time to further split up these figures into myopic, hypermetropic or mixed astigmatism, but the hypermetropia is by far the commoner variety on my books. But what is noticeable throughout is the very few cylinders comparatively that go "with the rule"—that is, at 90° in the case of hypermetropia and 180° in myopia; and a further factor of interest is that the further the angle of astigmatism is "from the rule" the greater is the severity and the chronicity of the headaches in comparison with the amount of the astigmatism.

Now in pretty nearly all these astigmatic cases of under 1 D. the vision without correction was $\frac{6}{6}$, but with a mydriatic in most cases the vision was reduced to $\frac{6}{12}$ or even less. This, to my mind, shows conclusively that the accommodation is trying to overcome the astigmatism, and therefore we have an irregular strain of an involuntary muscle, and that strain is the irritation to the sympathetic through the ophthalmic ganglion. A theory has been promulgated that the headache was due to the brain endeavouring to focus for itself a badly focussed image by the eye, but this is put out of court by the patients often affirming that the homatropine had relieved the pain for twenty-four or forty-eight hours, whereas the focus is made worse during the period of mydriasis. A number of my cases have had a certain amount of heterophoria which was an accessory factor in the cause of the symptoms, but in nearly all these cases the heterophoria was only secondary to the upsetting of the automatic balance between the intra- and extra- ocular muscles of accommodation, brought about by the strain of focussing and accentuated by the debility of the patient consequent on the recurrent headaches.

The further theory of ocular headaches being due to disturbances of the "fusion centres" is not borne out by my statistics, which do not show a large percentage of cases of anisometropia—in fact, a very small number, in all of which there is the factor of astigmatism—neither have I noticed any marked headaches in cases of paresis or paralysis of the third, fourth or sixth cranial nerves. We also find that where the astigmatism is great, say 2 D. or over, the symptom "headache" is rare, but vision is bad, because in these cases the accommodation is incapable of such an irregular strain as would obtain a good focus; and yet it is quite capable of overcoming an hypermetropia of 2 D. without astigmatism and therefore regular.

Headaches arising from irritation of other branches of the fifth nerve I will not here remark upon, but I would like to remind you that when the ophthalmic surgeon has eliminated all causes in his department and hands the patient over to a nasal or aural surgeon, it is the latter's business to find something, however small or insignificant, and if he fails, then to send the patient to a dentist or anyone except back to the ophthalmic surgeon.

There were ten cases in which there was astigmatism of only .25 or less, in debilitated subjects, which were not corrected by means of glasses. In young people who are below "par" I invariably order tonic and muscular treatment before ordering glasses, so that the accommodation, by being in better training, may be able to do its work without causing undue irritation.

The following notes of cases will serve to illustrate the points I have endeavoured to prove:—

(1) Mrs. C., aged 28:—Severe headaches, leading on to fainting and vomiting. Days in bed; dyspepsia; fullness of head; bad dreams. With homatropine, R.V. $\frac{6}{12}$ improved to $\frac{6}{6}$ *c.* + .75 cyl. at 35°, L.V. $\frac{6}{12}$ improved to $\frac{6}{6}$ *c.* + .50 cyl. at 175°.

(2) Mrs. M., aged 30:—Headaches about three times a week for some years; "hereditary" ! ! Vomiting and retching, and had often to go to bed for two or three days. V. $\frac{6}{6}$, reduced to $\frac{6}{18}$ with homatropine, and improved to $\frac{6}{6}$ *c.* + .50 *c.* + .25 cyl. at 90° in each eye. Interviewed a month ago, and says she is quite well while wearing the glasses.

(3) Mrs. A. W., aged 34:—Headache every day; bilious attack nearly every other day; when waking in the morning everything seems to whirl round, and waving lines are seen on the wall; whirling continues even when eyes are shut—this ends in an attack of vomiting, and then the headache improves; marginal blepharitis. With homatropine, R.V. $\frac{9}{12}$ improved to $\frac{6}{6}$ *c.* — .75 *c.* — .50 at 30°; L.V. $\frac{6}{12}$ improved to $\frac{6}{6}$ *c.* + 1 cyl. at 160°.

(4) Mr. S. C., aged 46;—Asthenopic symptoms and neurasthenia. Patient states: "I have acute headaches, which have come back to me on my return to Johannesburg from London, and for which I went to London. I have been in a very nervous state for some months, and in fact at times am unable to perform my business duties through inability to face work or worry of any kind. I am taking a tonic given to me by a brain specialist I saw in London, who ordered me to stop all business work and worry." I found this patient was

wearing smoked planos. On correcting with homatropine I found I could improve vision from $\frac{6}{18}$ to $\frac{6}{6}$ with + .25 + .50 at 180° in each eye, and added + 1.50 to above for reading and work. I frequently meet this patient, and he informs me that he has hardly had a headache since wearing his correction constantly; that his weight has increased 30 lbs.; and that he works for long hours, but always wear bifocals.

These notes of cases have been taken in Johannesburg, where the altitude of 6,000 feet must have some contributory affect on the action of the ciliary muscles, such as is well recognised to occur in the general muscular system.

DISCUSSION.

MR. WOOD said he was not able to agree with all in Dr. Brinton's interesting paper. He would remind members that out of the whole body there was only one organ where a scientifically exact diagnosis was possible, *viz.*, the eye, as regards its optical conditions. The result of this was that small optical defects might be discovered, and looked upon as the cause of troubles, to the exclusion of other more obscure but more potent irregularities elsewhere.

If they were to come to any accurate conclusion, it would be first necessary to classify headaches. The eye discomfort, the wearing ache, the sleepiness, and the pains in the eyes, behind them, or in the nape of the neck, associated with the use of the eyes in near work, could not be catalogued along with headaches coming on at any time, lasting for varying periods with freedom between the attacks, sometimes causing prostration and complete inability to work or even to eat.

The most important test was the coincidence of the headaches and the use of the eyes in near work. Where this was not proved the result of ordering correcting glasses was at least uncertain and had to be regarded as an experiment.

Again, a headache coming on at any time was rather unlikely to be caused by eye defects, and especially so if it was very severe. Complete freedom between attacks was also a rare condition where eye defects were the cause, and these again seldom produced the complaint frequently made of more or less continuous headache lasting for a few weeks or months in a person formerly free from it. They must, however, remember that in all these conditions there might be an optical defect which frequently had to be treated so as to obtain a diagnosis by exclusion.

It was necessary to remember that astigmatism was more frequently found among the patients of an ophthalmic surgeon than in those of a physician, and, furthermore, that large numbers of people are astigmatic and have no headaches, the defect coming to light only because failing sight or presbyopia necessitated a visit to an ophthalmic surgeon.

Besides all these sources of fallacy they had to beware of claiming cure unless proved by a later examination in all cases of headache where an optical defect existed, because the cases where relief was not afforded as often as not merely tried elsewhere.

He believed that where astigmatism caused headache it did so largely in proportion to the effect it had in making the patients, or at least the younger ones, read at too short a range, and he had seen very many cases where a low degree of astigmatism had been exactly corrected by a colleague, but where the headaches continued, because the patient was merely suffering from the fatigue due to an improper reading distance. He thought that this point should have great stress laid on it, as what he called "nose reading" was one of the commonest causes of headache in young children. In this connection he would like to say, with much deference, that the condition described by Mr. Swanzy under the title nervous amblyopia in neurasthenia was in most instances the syndrome induced by an improper reading distance. They might have read in the *B.M.J.* (August 13th, 1910) an admirable lecture by Mr. Doyne on "The Use and Misuse of Spectacles," with the most of which he was in complete accord. There was, however, one symptom which Mr. Doyne claimed as produced by ophthalmic abnormalities, *viz.*, polyuria. This Mr. Wood believed was much more frequently, if not always, associated with that most potent cause of headache, toxæmia from slowing of what might be called the digestive cycle. He had gone into the statistics of his last thousand cases of refractive errors and found that, whereas headache was the main complaint in 119, no fewer than 600 of the cases showed astigmatism. Of the 119, 26 were cases of astigmatism, 21 read at too short range, 11 were cases of deficient convergence and were helped by prisms, 10 were cases of what he called "sun headaches," 10 were cases of auto-intoxication, 6 had hyperphoria, 5 had typical migraine and 5 has nose troubles.

He did not want to lay undue stress on these figures, but they would observe that they were not even of the same order as those given by Dr. Brinton, and it was evident that a much more careful classification was needed before statistics could be relied upon.

In conclusion, he would like to point out how many cases of headaches were due to retinal hyperæsthesia. In the bright light of South Africa the prolonged overaction of the orbicularis and corrugator supercilii, when the patient went out of doors, produced just such a headache as did an optical or an eye muscle defect. In such cases the only means of relief lay in the use of tinted glasses.

Dr. Caiger rather agreed with the President's views, and thought that many cases of vague discomfort, not related to the use of the eyes, were due to uncorrected slight astigmatism, and quoted Dr. Gould of Philadelphia in support.

Dr. Luckhoff was inclined to agree with Mr. Wood. He said he intended to pay more attention to patients' reading distance and the effect of bright light, and raised the question as to whether it was mainly the actinic rays which were the cause of the discomfort felt in the cases alluded to.

Mr. Wood thought it was partly due to the short wave rays, but had seen no superiority in glasses designed to cut off these rays only over the ordinary neutral tint which degraded the whole spectrum.

II.—DIAGNOSIS AND TREATMENT OF HEADACHE DUE TO DISEASE OR DISTURBANCE OF EYE, EAR, THROAT OR NOSE.

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(*Special Subject for Discussion*: *Second Paper.*)

I intend to arrange the subject under two headings, *viz.*, Diagnosis, etc., of the Causes (*a*) External, and (*b*) Internal to the Cranium.

The nose, naso-pharynx and ear are in reality one cavity with passages leading to various annexes, and all are lined by a continuous mucous membrane. The floor of the frontal sinus forms a portion of the roof of the orbit, the supraorbital nerve running in a notch or groove in the bone; the ethmoidal cells have openings which permit of free drainage—if not blocked; the cribriform plate separates them from the dura mater of the anterior fossa of the cranium. The great risk, if they become diseased and their openings are occluded, of an upward extension producing a meningitis can be easily understood The anterior cells of the ethmoidal labyrinth are completed by the lachrymal bone, and the proximity of the lachrymal sac tends to easy infection. The floor of the orbit is partly formed by the orbital plate of the superior maxilla, which at the same time forms the roof of the antrum; the second division of the fifth nerve runs in a groove or canal in this plate, hence the frequency of neuralgic pain when the antrum is affected. The sphenoidal sinuses are in close relation to the base of the brain. These cavities, when large, may undermine the sella turcica and come into close relationship with the optic nerves: laterally to them are the cavernous sinuses and their contents; the bony wall separating them from the cavernous sinuses may be extremely thin, and even absence of parts of the walls has been recorded.

In the ear we find the tympanum, a small cavity at the end of a tube, whose lumen very easily becomes occluded and is badly situated for drainage. The iter ad antrum is high up on the posterior wall of the tympanic cavity, and situated badly for drainage from the antrum; and lastly, the mastoid cells of that process are absolutely in a dependent position. The proximity of the sphenoidal sinuses to the cavernous sinus, the thin, perforated cribriform plate separating the ethmoidal cells from the dura mater of the anterior fossa, and the proximity of the sigmoid sinus to the mastoid antrum and antral cells, cause one to seriously consider the great possibility of implication of these unfortunate neighbours. A few of the important nerves have been already mentioned, and I will remind you of the relationship of the facial nerve to the tympanic cavity, and the importance of a small plexus, the tympanic plexus, situated upon the inner wall of the tympanum.

In "A Review of the Sensory System of the Facial Nerve and its Symptomatology," by J. Ramsay Hunt, M.D. (*Journal of Laryngology*, August, 1910), he suggests the following reconstruction: "The facial or seventh cranial nerve is composed of two roots, motor and sensory, which unite at the level of the geniculate ganglion, which is situated in the depths of the internal auditory canal, at the entrance of the aqueduct of Fallopius, at which point the trunk of the facial nerve makes a sharp bend. On the distal side of the ganglion the nerve is divided into three branches or divisions analogous to the terminology which has been assigned to the trigeminus on the distal side of the ganglion. The first division is the great superficial petrosal nerve passing to Meckel's ganglion and giving off in its course a tympanic branch. The second division is the small superficial petrosal nerve passing to the otic ganglion, and giving off in its course a tympanic branch. The third division includes the motor trunk, the chorda tympani, and the sensory fibres destined for the external ear. Around this third division is grouped the well-known symptomatology of Bell's palsy."

It certainly serves as a practical basis for clinical descriptions, and gives to the sensory mechanism of the seventh cranial nerve the importance which it deserves. The facial nerve, therefore, occupies an essential part in the innervation of the auditory mechanism, and when one considers the very sensitive structure of this mechanism, and its exposed situation, it is not surprising that neuralgic affections of this system, owing to its many connections, are of frequent occurrence.

The nerves of the nasal organ are the olfactory, the nasal branch of the ophthalmic division of the fifth cranial nerve, filaments from the anterior dental branch of the superior maxillary, the vidian, nasopalatine, descending anterior palatine, and nasal branches of Meckel's ganglion.

A.—Headache due to Extra-Cranial Causes.

Furunculosis of the columna nasi, the central portion of the upper lip, and within the external auditory meatus, gives rise to the most excruciating pain locally and a diffuse headache. The binding of the integuments to the deeper structures by fibrous bands prevents general expansion and thus diminution of tension. It is unwise to wait for the formation of pus in these cases, and as soon as a definite circumscribed area of congestion or inflammation presents itself a free incision with a tenotomy knife affords the quickest relief,—a small puncture or the injection of abortifacients, *e.g.*, tr. arnica or carbolic acid, seems to me worse than useless. When such areas of inflammation open naturally they as a rule do so by small multiple orifices, and are more of the carbuncular type than the ordinary furuncle.

Abscess of the septum nasi very frequently follows such nasal inflammation, and produces a large unilateral or bilateral swelling, extremely tender, either localised or diffuse, and severe headache. The nasal mucous membrane over the septum is thick and tough, and it takes a long time for the natural opening of the abscess to form. On looking into the nose it is easy to mistake such a swelling for a deviated septum, unless a probe or the little finger point is introduced, proving fluctuation to be present by sight or touch. I have seen one half of the nose practically occluded by such swellings.

A long incision is the only treatment, and that done as early as possible. The extremely tender part is best operated upon under gas or without an anæsthetic. Local anæsthesia causes as much or more pain than the incision itself, and it is only natural that the injection of a solution of a local anæsthetic into a part already tense and admitting of little further expansion—and certainly not a rapid one—coupled with the needle puncture, must be productive of more pain than the quick use of a sharp knife.

Furunculosis of the external auditory meatus often leads one to think of the possibility of mastoid disease. A small discharge may be present from the furuncle, and the signs of pain in the region of the mastoid may be misleading. Examination *per speculum* may help us little, for it is very often impossible to introduce it, owing to the swelling, quite apart from the pain produced thereby; pain upon mastication and localised tenderness over the mastoid must to a great extent be relied upon. Even the fact that a discharge was present beforehand does not eliminate the possibility of the pain being due to the furunculosis, for one is more likely to have a localised furuncular infection if the tissues are being bathed with pus from a discharging otitis media suppurativa; finally, they may occur simultaneously. The differential diagnosis must be carefully made after considering the following points: Furunculosis occurs suddenly, in contrast to acute mastoid disease, which usually follows an acute middle-ear suppuration; in furunculosis the pain on mastication and movement of the auricle and manipulation of it are prominent characters, whereas they are not so in acute mastoid disease.

If they have both occurred together, after the furuncle has been incised and treated, subsidence of symptoms should follow, whereas a continuance of the symptoms points to mastoid involvement. It should be mentioned that pyrexia and pain, though present in both, vary according to the freedom of exit for the discharge. When a furuncle has been opened and is discharging, the pyrexia and pain subside; so with an acute mastoid, if the discharge is free into the tympanum and through the membrana tympani, pyrexia and pain are slight. Should the membrana tympani not have perforated the pyrexia remains high and the pain severe; so, in all cases where there is the probability of the two diseases being present, I lance freely any swellings within the canal, and if after twelve to twenty-four hours there is a continuance of pain and pyrexia, the great probability of deeper involvement must be considered.

In acute mastoid disease pressure over the mastoid process elicits much more pain. In both the auricle may be swollen, and projects more outwards from the head; if the back aspect of the head is viewed, it will be noticed that in acute mastoid disease there is asymmetry of the two sides behind the auricle, and that the auricle projects downwards, forwards and outwards. The external auditory meatus may show marked elevations in its canal, whereas only a general swelling or thickening may be noticed in acute mastoid disease If a view is obtained of the membrana tympani, it is found intact in the one and bulging or perforated in the other, and the discharge may be seen to pulsate; deafness is as a rule marked in acute mastoid disease following acute otitis media, but unaltered in furunculosis. One is also better able then to obtain a view of the membrana tympani, or perhaps to see a bulging downwards of the upper posterior part of the auditory canal, indicating an involvement of the cells of the anterior wall of the antrum and mastoid process.

Adenoids are a very common triple cause of headache. There is always hypersecretion, which trickles down the nasopharynx into the œsophagus and is swallowed; this coats the mucous membrane of the stomach and prevents the proper action of the gastric juices, and so causes stomach derangement with subsequent anæmia and headache. Most subjects of adenoids or nasopharyngitis are anæmic and suffer from headache. Eustace Smith advises the administration of ferri sulph. or zinc. sulph. gr. 20 in a small quantity of water in the early morning, whereby vomiting is produced and a large amount of mucus is ejected. This is followed by immediate relief of the headache, and temporary improvement of the anæmic condition; it is usually given for six mornings in succession.

Adenoids, besides causing headache in this way, block the entrances of the Eustachian tubes, the absorption of the contained air follows, and negative pressure headache is the result; later the vessels within the tympanic cavity, being subjected to less pressure, permit of exudation with its accompanying fulness, impaired hearing and headache. They lastly increase the liability of a nasopharyngeal catarrh spreading up the tubes and causing infection of the tympanic cavity and its annexes, which account for the greatest number of serious headaches due to disease of the nasopharynx and ear. Very often do we see cases of serous accumulation in the tympanum rapidly disappear after catheterization of the Eustachian tubes, merely because the equilibrium between internal and external air has been again secured. How often do we see recurrences of this condition until the adenoid condition is removed! The operation is simplicity itself, and I only add one point—that there is no necessity to adhere to the old rule of making one's operative field between the Eustachian orifices; by all means do the vault and posterior nasopharyngeal wall first, but it is quite an advantage to the patient to curette gently the lateral aspects, especially that little space—Rosenmüller's fossa. Never use a sharp curette, or shall I say an oversharpened one.

Another simple cause of headache is *impacted cerumen*. It is by no means exclusively seen in the neglectful, but often in those most careful of their toilet. The farmer is a frequent subject, and another is the young lady. Pain amounting to acute headache is felt chiefly when the mass swells after a bathe. The association of the headache with the offending mass is proved when it is removed first by instillation of warm hydrogen peroxide solution (for without this the proceeding is often exhausting), and the subsequent syringing with warm water to which bicarbonate of soda is added or antiseptic ethereal soap.

Conchal perichondritis gives rise to most intense pain, at first localised to the part, but subsequently diffuse, with exacerbations of neuralgic pain. Do not wait until the tissues are tense; in most instances the swelling is due to serous exudation, and if multiple small incisions, not deep, are made, much relief from pain and subsequent deformity will result. Mop the auricle with antiseptic solution previous to incision, and subsequently apply lead lotion. It is frequently associated with furunculosis of the external auditory meatus.

The Annexes of the Nose and Ear.—The mucous membrane of the nasal fossa is continuous with the conjunctiva through the lachrymal canal, and with the mucous membrane lining the various sinuses through the orifices leading to them, including the tympanum through the Eustachian tube. Acute rhinitis is a frequent symptom of certain of the infectious diseases, *e.g.*, measles, influenza, scarlet fever, small-pox and diphtheria. At the onset of an attack pyrexia, dryness and fulness in the nose, throbbing pain in the forehead, huskiness of the voice, diminished sensation of taste, smell, and hearing, all point to a general involvement of that continuous lining. These symptoms may in a few days subside, or the catarrhal condition may persist at one or other part, giving rise to a different train of symptoms with general headache, frontal headache, maxillary pain, etc. An acute rhinitis is generally the starting-point of trouble in the various sinuses—frontal, maxillary, ethmoidal, sphenoidal, lachrymal and tympanic.

Fortunately most of these acute attacks subside within a few days, but frequently a patient comes to us with the history of having had an attack of acute rhinitis with acute headache in one or other special region and with nasal discharge, which discharge persists though the headache is only occasional. The sites of these sinus headaches vary very much, nor are they always present. In cases of frontal sinusitis the frontal region is the common site of headache, for it is supplied by twigs from the supraorbital and nasal nerves. It may, however, be felt in the occipital and temporal regions on account of the nervous connections made with the nerves of those regions.

In maxillary sinusitis pain is most commonly felt in the cheek over the cavity and in the teeth; the mucous membrane lining the antral cavity receives filaments from the infraorbital nerve by means of its superior dental branches. In ethmoidal sinusitis the headache is most frequent in the nose, forehead, and eyes; the air cells are supplied by minute branches from the nasal nerve. In sphenoidal sinusitis headache is most felt as a jolting, jogging sensation at the centre of the head behind the eyes. This sinus is supplied by filaments from the spheno-palatine ganglion.

Headache varies in sinus affections; at times it is absent, at others very acute. This can be accounted for by the patency or otherwise of the opening and the amount of accumulated discharge in the sinus. The headaches are always most severe in the mornings. The openings to all the sinuses are small, and, with the exception of the frontal, badly situated for drainage; congestion of the mucous membrane at the openings is sufficient to completely occlude them. Then follows absorption of the contained air with diminution of the atmospheric pressure, and a negative pressure headache is felt; subsequently there is an exudation into the cavity, and pain is then often intense—tension headache. Acute pain is chiefly felt in acute sinus involvement, and more of a dull aching in chronic affections, with acute attacks from time to time due to blocking of the openings by polypi and crusting and hypertrophy of the mucous membrane. As a rule one sinus is found involved, but multiple sinusitis may be present.

Diagnosis of Sinus affected.—This can be made in many ways. The routine method of inspection with a nasal speculum and a good light is first undertaken and supplemented by catheterization, puncture and transillumination.

General diagnosis: (1) Pus seen in the middle meatus, the patient sitting upright, indicates that it may come from either the maxillary antrum, frontal sinus or anterior ethmoidal cells, *i.e.*, the anterior group of sinuses. (2) Pus seen in the olfactory fissure above the middle turbinal indicates that it may have come from either the sphenoidal sinus or the posterior ethmoidal cells, *i.e.*, the posterior group of sinuses.

The subject of diagnosis will be found fully discussed in my paper, *Transvaal Medical Journal,* August, 1910.

Ethmoidal disease is the most difficult to treat, even if seen in the early stage, which is natural, owing to the many small cavities which one ought to freely open, but which it is impossible to do thoroughly per anterior nares. Nasal douching with the usual solutions has not much effect beyond the clearing away of accumulated secretion. In recent cases which resist treatment up to three months I remove the middle turbinate as high up as possible, and, bearing in mind that my operative procedure is limited to a very small space bounded by the os planum externally, by using bluntish hooks and with caution and little force I tear away by a series of half a dozen perpendicular slices the space between the removed turbinate and the os planum. Bleeding is free even after a thorough application of adrenalin. It is possible to do a great deal without passing the reasonable limits of pain in a patient, not unusually hypersensitive, by patience, frequent mopping with hydrogen peroxide and adrenalin, and finally douching or coarse-spraying with warm hydrogen peroxide solution.

Radical operations in many cases will have to be performed, but it is impossible to enter into this long subject here. I firmly believe, firstly, that my views as expressed in my paper on the accessory sinuses of the nose—" A Plea for the Early Treatment of Cases of Rhinitis following an ordinary ' Cold in the Head ' or the Infectious Diseases "—will assist in reducing the number of such operations; secondly, that when a case has not existed more than three months, by maintaining the patency of the openings to the various sinuses so that air is freely admitted and drainage permitted, by minor operations, *e.g.,* removal of polypi and hypertrophied parts, attention to abnormalities of the nasal fossa, which are possibly interfering with the big airway and drainage system—the major operations would be still further reduced.

The tympanic cavity or sinus probably causes the most acute headache of all the annexes. Its length of air-passage leading from the nasopharynx renders it liable to obstruction through similar affections attacking the nasopharynx. Occlusion of the Eustachian tube leads in the same way to absorption of air and negative pressure, discomfort or headache, and exudation into its cavity leads to tension headache. Any increased tension within the cavity by pressure on the tympanic plexus leads to acute agony. It is at first localised to the ear, but soon spreads up the side of the head, down the neck and over all the branches of the fifth nerve. In acute otitis media this is best seen and felt. Bearing in mind the fact that whenever the passages leading to the various annexes is made patent, permitting free drainage and admitting air, the headache gradually or abruptly ceases, so here it should be our object to render such treatment to the nasopharynx by means of warm solutions of alkaline antiseptic washes and then applying to the orifice of the Eustachian tube, through the inferior meatus, a solution of adrenalin, 1 in 1,000, followed by auto-inflation or catheterization. I am aware that I have touched upon a very debatable treatment, but I have seen many cases where an acutely painful otitis media with bulging membrane has been markedly relieved in the space of an hour or two by this treatment, and the admission of a bubble of air into the small tympanic cavity has caused rapid absorption of the contents. Sedative instillations, cocaine and carbolic acid in glycerine, and internal remedies may at the same time be advantageous. Paracentesis of the membrana tympani, having previously most carefully cleaned the passage of the external auditory meatus by antiseptic solutions, is the favoured treatment; the subsequent protection of the incised part by a small sterilized pledget of wool is necessary.

The earache of infants is practically always due to acute otitis media, and when severe may closely simulate meningitis. They cry, roll about, put their hands to their heads or ears, and resist being touched about the ear; they may have convulsions, head retraction, vomiting and all the symptoms of meningitis. A perforation takes place at last with subsidence of the acute symptoms. In all such cases examine the ears of infants and young children, and either inflate them by catheterization, which is easier the younger the infant, as the trumpet-shaped ends of the Eustachian tubes are in them large and markedly patulous—I have seen the immediate disappearance of an accumulation in the tympanum follow it, with immediate subsidence of acute headache; or perform paracentesis. Remember that it is important to relieve an acute otitis media in an infant with head symptoms, for the petro-squamous suture permits of free communication between the blood-vessels and lymphatics of the tympanic sinus and the middle fossæ of the cranial cavity.

The headache of an acute otitis media is in most cases relieved by perforation or paracentesis of the membrana tympani, but not always, however. The headache may not subside although the discharge may be free; here the possibility of the simultaneous involvement of the mastoid cells and deep auditory apparatus, or the intracranial cavity itself, must be seriously considered. In the cases where the opening in the membrana tympani has become occluded by granulation tissue or dried pus owing to the smallness of the opening, with reappearance of the headache, by opening it up again and giving a free exit to the discharge, the headache and pyrexia will be relieved. One may temporize if necessary, if it is found that the acute mastoid disease has been provoked by occlusion of the opening; under no circumstances, however, may we do this when the discharge is free, pyrexia high and headache continuous. If one temporises, soothing applications to the head—hot or cold—warm douching with large quantities of hot water or a hot solution of boracic acid, may assist the relief of pain and render the acute symptoms quiescent. In the other cases operative measures of a radical kind must be undertaken, and the requirements will vary in every case. Perhaps the complete operation—modified—or a further extension of the operation into the intra-cranial cavity, will be necessary.

Pressure or contact headaches are very common, and with mild operative measures relief and cure can be obtained. They may be due —(*a*) to a markedly deflected septum coming into contact with the middle and inferior turbinates or with one only; (*b*) the septum may be quite normal, and one or other of the turbinates come into contact with it through hypertrophy; (*c*) the middle turbinate may, by means of the deflected septum, be crushed down upon the inferior turbinate or curled up against the outer wall of the nasal fossa; (*d*) a spur, frequently found at the site of the junction of the triangular cartilage of the nose with the vomer may press against the inferior turbinate.

In all these cases there is more or less constant contact, and they are most frequently seen in those long narrow noses, the pinched type of nose, seen in adults who have had adenoids in childhood and in whom the natural growth or overgrowth of one or other of the parts has not found sufficient space. Headaches felt in the forehead, lachrymal area and in the temple on the side of the contact are most frequently complained of. They are of the dull aching kind, and can be temporarily relieved in most cases by the application of cocaine and adrenalin.

The necessary operative procedure in each of the causes mentioned under *a, b, c, d,* is obvious. A markedly deflected septum should of course be rectified by submucous resection, the hypertrophy of one or other turbinate removed by turbinectomy or a spur removed.

During the cauterization of hypertrophied turbinates—commonly the middle—with the intention of binding the mucous membrane to the deeper structures, parts opposite to each other on the septum and turbinate are frequently cauterized and followed by an adhesion between the cauterized parts. This contact by means of adhesion I have seen followed by headache and relieved when a portion of the middle turbinate had been removed, the scars cicatrising without contact.

Vasomotor headaches.—Patients frequently inform us that they suffer from nasal obstruction as soon as they lie down at night, and that in the morning they wake up with headache and a dry tongue, which passes off after being up a few hours, or that they suffer immediate headache, stuffiness and obstruction in the nose if they walk about in the cold bare footed, or that a nasal obstruction, or headache, and watery nasal discharge comes on as soon as they become nervous through any cause—*e.g.,* calling—and that the frequent use of the handkerchief under such circumstances is most inconvenient to them. On examining the nose in such cases it will plainly be seen where the turbinals come in contact with the septum by a distinct ridge on the septum. It is most commonly seen in women, but frequently in men. The treatment of such cases is tonic, plus the binding down of the mucosa to the bone by cicatricial tissue—best done with a probe-pointed cautery,—the nose being well anæsthetized by cocaine 20 per cent.—or by horizontal cauterizations which must extend deeply, as the object is to form cicatricial tissue, and thus prevent the expansion of the cavernous tissue of the turbinals.

Neuralgic Headaches.—Supra-orbital neuralgia, apart from frontal sinus disease, is commonly seen in cases of nasal polypus. A very painful neuralgic affection of the nose and inner canthus of eye is due to disease of the lachrymal cell or sinus. The lachrymal bone completes the anterior cells of the ethmoidal labyrinth, and this painful neuralgic affection is really part of a general ethmoiditis. Occipital neuralgia and headache are common in cases of Eustachian obstruction and chronic catarrhal otitis media with adhesion of membrana tympani to the promontory. It is relieved temporarily by inflation of the tympanum.

Reflex neuralgia in the region of the ear is frequently seen in cases of quinsy, ulceration of tonsils, etc., and acute reflex neuralgic ear pain is commonly seen in cases of tubercular and malignant laryngitis.

B.—Headache due to Intracranial Causes.

Sinus thrombosis, meningitis, extra-dural abscess and cerebellar abscess—all have headache as a symptom. If it is known that there is or has existed a chronic middle-ear suppuration on the same side, then this symptom is of grave importance. Pain on pressure over the mastoid, rigors and vomiting are the usual indicators, coupled with the knowledge of chronic middle-ear suppuration, that thrombosis has taken place in the lateral sinus. The onset is sudden, the rigors are a prominent feature, but the headache is not very marked. In meningitis the headache is of the dull kind, the pyrexia is irregular, and there are the usual signs of cerebral irritation. In cerebral abscess the headache is most acute, and is accompanied by vomiting, optic neuritis, slow pulse and subnormal temperature. Treatment of these cases may be undertaken through the site of the complete mastoid operation, from whence exploration of the various regions may be made and the necessary treatment adopted.

III.—THE TREATMENT OF OPHTHALMIA NEONATORUM.

By A. M. Baumann, M.D. (Leipzig), Bloemfontein.

The question was put to me one day by a medical friend of a very observant nature who has a large native clientèle, "How many cases have you seen of corneal changes which could be attributed to the ravages of ophthalmia neonatorum?" I had at one time a fairly large native practice, and even now see a fair number of coloured people. I could recall no case of ophthalmia neonatorum which had been brought to me for treatment by natives. I must here acknowledge that to Dr. Friedman, of Bloemfontein, I am indebted for the explanation of the facts. He read a paper on the subject at the O.F.S. Medical Society in Bloemfontein, an extract from which I give later on. I was eager to learn what was the experience of others on this same subject, and made enquiries of five other practitioners who had had a large experience amongst natives.

I got the same answer from all, *viz.*, that they did not remember seeing such changes. I will call my informants (*a*), (*b*), (*c*), (*d*) and (*e*). (*a*)—A practitioner in Basutoland, wrote: "Although gonorrhœa is very prevalent in Basutoland, cases of ophthalmia neonatorum are very rare. During sixteen years of practice in that country I have only seen two cases, and these came for relief at once. Considering the rarity of the disease, I can only write that I have observed no corneal changes I could attribute to past ophthalmia neonatorum. That there is not more of it has been a puzzle to me. Corneal diseases are rare in this country, with the exception of interstitial keratitis; ulcers are very infrequent." Regarding treatment of the two cases, the answer I got was that hydr. perchlor. (1 in 10,000) was used, and no argent. nitr. was required.

(*b*)—Also practising in native territory, told me he had seen no changes he could attribute to past ophthalmia neonatorum, but that he had seen a good many corneal changes which he attributed to syphilitic causes. No cases of ophthalmia neonatorum in natives had been brought to him for treatment.

(*c*)—Dr. Friedman, with an exclusively native practice near Bloemfontein, had seen no corneal changes attributable to past ophthalmia neonatorum. An extract from his paper is as follows:

"In my practice among natives, which extended over a period of nine years, I have been consulted only four times for that disease, most probably by young mothers who had not sought the advice of the old experienced women. Altogether I have attended twenty-two cases of blenorrhœa neonatorum, eighteen of which I saw on the occasion of a visit paid to their ill mothers; seven of these cases were without doubt, the remaining cases more than probably, of a gonorrhœal origin. Now all these cases healed completely without any medical assistance, and did not leave behind the slightest mark on the cornea. Struck by this fact, I paid special attention to the cornea of natives, and, although in former years thousands of them used to pass through my hands annually, I never had a single case of leucoma on the cornea or other permanent injury to the eyeball as the result of blenorrhœa neonatorum."

He further said that in his opinion the cornea of the native does not show a greater resistance to infection than that of the whites, the cornea often becoming affected by relatively harmless catarrhal conjunctivitis.

(*d*)—Whose practice is largely amongst natives, and he having a good deal of obstetric work, had also seen no such cases attributable to this cause. Native infants were sometimes brought to him with what mothers called "zwell zeer ooge" (swollen sore eyes). He gave a weak hydr. perchlor. solution and argyrol drops.

(*e*)—Who has a large native practice, answered as follows: "I have seen quite a number of eyes completely blind, but I do not know that I could state whether it was gonorrhœa or syphilis. I have only had one case of acute ophthalmia neonatorum and treated it with silver nitr. and 10 per cent. protargol. My experience as to gonorrhœa is that it is not at all common among the natives, especially the country natives."

Now what can be the cause of this remarkable fact? The first thing one would of course think of would be immunity of the native to gonorrhœa. Immunity against gonorrhœa partial or complete does exist; I think we have all met with such cases. I can remember at least three cases where I have good reason to presume whole or partial immunity. But that this is not more frequently the case amongst natives than amongst whites is proved by the experience of most of those who have practised amongst them. Only one of my informants states that gonorrhœa is not very prevalent, whereas the experience of five others, including my own, is in favour of gonorrhœa being as prevalent, or very nearly so, as amongst white people. My Basutoland informant states that gonorrhœa is very prevalent in that country. From Thaba 'Nchu district I hear that though gonorrhœa may not be quite so common as amongst whites, still it is very prevalent amongst them. My town colleagues, as well as myself, have a similar experience as to the prevalence of this disease amongst natives. Therefore I think we can dispose of the immunity theory. We must conclude that as many mothers have gonorrhœa, we should expect many new-born children to have ophthalmia neonatorum. Then why do we not see these cases or their after effects? Could it be that they are cured in spite of, or because of, no treatment?

We may say, why are these children not brought for treatment, as natives have great faith in their medical men, provided they give them careful attention? The case I presume to be this: Perhaps being far away from medical help, and unwilling to travel with their children, the mothers may have noticed that the children's eyes got well without being treated; that after a lapse of some days or weeks the eyes were opened, a little later the discharge stopped and the eyes were well. After repeated experiences of this kind, the tradition may have arisen that this state of swollen eyes was normal to some children or due to a cold, and in consequence disregarded as of no importance and sure to heal by itself. The infant, of course, might be well in every other respect, and need not be troublesome in any way, so that there was no need to call in a doctor.

I cast about in my mind for some experiences of my own of analogous cases in whites and other coloured people—not necessarily pure natives. Some occurred to me. About seventeen years ago I was called to attend two white children, brother and sister, of respectively 8 and 12 years, who were suffering from what I could only class as gonorrhœal ophthalmia. No bacteriological examination was possible, so I was forced to base my diagnosis on clinical symptoms alone.

The lids were swollen and very hard. I could not see the cornea till a much later date. Later on there was a very profuse purulent discharge. I directed the eyes to be bathed with, I believe, boric lotion and argent. nitr. drops, 2 grs. to the oz. Neither of the drugs could have entered the conjunctival sac for at least a week, as it was absolutely impossible to open the eyes. Gentle means failed to give me a view of the cornea, and I refrained, from cowardice, to use retractors, and did not even slit the outer canthus. I was in a very anxious state of mind for more than a week. I pictured to myself an ulcerated cornea, a prolapsed iris, etc. After maybe eight days, they were able to open the eyes, and to my delight revealed a bright uninjured cornea. Then the argent. nitr. could be dropped in; before this, assuredly nothing had entered the eyes. The little native child got no treatment, and I never heard that it was any the worse, for had any corneal change taken place the child would have been brought to me. This, of course, was in my pre-ophthalmic days.

Shortly after this I had another case. A bastard Hottentot girl was pregnant by a man of the same colour. The man had gonorrhœa and prostatitis, and was under my treatment; the girl owned to having leucorrhœa. I warned her that the child would have sore eyes. She informed me, when the child was a few days old, that it had bad eyes, but I was not asked to treat it. I prescribed some zinc drops, but do not know whether they were ever used; I suspect not—at any rate the eyes did not suffer.

My informant (*b*) in district Thaba 'Nchu gave me a very illustrative experience. During the war he saw two cases of ophthalmia neonatorum in white infants. The one was the child of respectable parents, who assiduously carried out the treatment prescribed, so they averred; this child showed ulceration of cornea. The other was the child of a loose woman, who rather laughed at the idea of doing anything, and really did nothing. The child recovered with no damage to the cornea.

Two other cases recur to me which happened within a year of each other. One was the child of better-class parents, and had the advantage of treatment from, firstly, the family physician, secondly a very able ophthalmologist, whilst a qualified nurse was in attendance to carry out the treatment. The cornea of the one eye ulcerated, and when I saw it there was a central leucoma. This child certainly had as good attention as the country could offer.

A few months later a case came under my care: it was the child of a prostitute born in a rescue home. I had it under treatment from the second or third day of illness. I could just catch sight of one cornea. I did not try to see the other till the child opened its eyes in the second week. The treatment was bathing the eye with boric lotion; the eye only opened as far as traction over the eyebrow would permit, and no other interference of the eyes was permitted. This was frequently done, and argyrol 3 per cent. instilled after sponging. No other treatment was used, and in little over five weeks the child was discharged cured, with an intact cornea. These two cases occurring close together are rather significant. I used the argyrol for reasons I will state later; besides, at that time I had not quite the courage of my convictions. Two more cases remain for me to report. One was the child of a native prostitute. The child had been born, I think, in a lavatory, and left there during the whole of a very cold winter's night. The child came under my treatment on the second day of illness. In this case I did not try to see either cornea. Here also the nurses were instructed not to touch the eye, but just to use slight traction over the eyebrow, which easily everted the very swollen conjunctiva. Again the eye was not touched with the cotton-wool swabs saturated with boric lotion. These swabs were squeezed and only so much of the discharge as could be got rid of in this way was removed. This was done at two-hourly intervals at first. In the second week I substituted sterilised water for the boric solution. In thirty-three days the child was discharged cured.

About this time a young man of 25 was sent to the hospital. He had gonorrhœa, and during four days the left eye had been affected. It presented the typical appearance of gonorrhœal ophthalmia. In this case I could see the cornea, which was fairly embedded in an intensely chemotic conjunctiva. In this case the eyes were not touched by the hands. An irrigator filled with tepid boric solution was held, above the level of the eye, the nozzle held near the inner canthus, and the patient directed to open his eye as much as he could, which was very little. The lotion was allowed to run at a very low pressure so as to prevent much movement of the lid. After this argyrol was instilled (5 per cent.). At the end of four days the discharge had greatly lessened and the chemosis had nearly disappeared. In the middle of the third week I substituted nizin for argyrol, and by the end of the fifth week the eye was practically cured. For other reasons the patient had to remain in bed. I must add that in both these cases on about the fifth day I had the number of irrigations reduced to three a day.

In these three cases the time of healing *ad integrum* was four to five and a half weeks, which will compare with the results of more energetic measures. In view of the facts I have mentioned and the cases which I have quoted it seems to me there is but one conclusion to be arrived at, *viz.*, that the reason why one meets with no corneal changes attributable to ophthalmia neonatorum in natives is that the eyes have been left strictly alone. My cases bear this opinion out in so far as they received a minimum amount of active and medicinal treatment, for one can hardly hold that boric acid solution or sterilised water have any action beyond that of washing away the secretion. Argyrol certainly has little astringent action, and according to laboratory findings very little antiseptic value; it may, however, have some influence upon the gonococcus. However, I used it in two cases for its undoubted soothing action in a congested state of the conjunctiva; although the case in which I used only boric solution and distilled water ran a shorter course than the other two. You will have noticed that my informant (*a*) used only hydr. perchlor. solution (1 in 10,000), practically only sterilised water, and cured his case with that.

I can think of no other solution which would explain the facts I have laid before you. On consideration this solution seems a very feasible one. In contradistinction to the conjunctiva, which offers a splendid nidus for the gonococcus, the cornea seems to have a considerable amount of resisting power to it. If it were otherwise, what chance would the cornea have of escaping infection in any case, bathed as it is for days in gonococcal pus? No cornea would escape. To what protective agency are we to ascribe this immunity but to its epithelial covering? So long as the epithelium is intact, the cornea is safe. When once the continuity of the epithelial covering is destroyed, there is no further hindrance to the injurious action of the gonococcus, and of course the unique nature of the nutrition of the cornea and its anatomical structure then render it an easy victim to the infection. We have another example of the highly protective power of the corneal epithelium. In cases of chronic dacryocystitis, virulent pus is discharged into the conjunctival sac for years maybe, and coming into daily contact with the cornea, leaving it intact; let however the slightest abrasion come about, and the cornea at once is susceptible to a most virulent form of ulceration.

The great danger in gonococcal blenorrhœa is admittedly corneal ulceration, and seeing that this disease, with but few exceptions, can only be due to an injury to the cornea, it is obviously our duty to avoid any possibility of this accident occurring. The exceptional cases I refer to are those of lowered vitality, as, for instance, in some cases of congenital syphilitic infants who have also a very good chance of contracting ophthalmia neonatorum. The whole vitality of the body is lowered, and with it that of the corneal epithelium, which in this case might give way to the macerating influence of the discharge, opening the way to infection. In these cases nothing could save the cornea.

In the extreme swelling of the lids we see Nature's endeavour to fight the foe. She has a very favourable condition in the extremely loose tissue of the eyelids which enables her to send an enormous amount of material to oppose the enemy. This swelling is supposed to endanger the cornea by causing pressure sores. I doubt this. Pressure bandages, which exert much greater force than the swollen lids, are sometimes applied to the eyes, but I have not heard of their causing pressure sores. We are taught that the chemosis endangers the nutrition of the cornea. I cannot conceive how this is possible.

We bring about this condition artificially by subconjunctival injections or by dionin: why should it in this case have the opposite effect? I mention this fact because pressure is supposed to cause sores and chemosis, to favour this by strangulating the sources of nutrition. We are told that cases of gonorrhœal ophthalmia in adults are more dangerous than in infants, and rightly so. This bears out my contention. When an adult feels anything in his eye, he rubs it; even we who know better yet do so. The extreme irritation of commencing gonorrhœal blenorrhœa excites correspondingly energetic friction. I think we have all seen these abrasions in milder forms of infection where it is easier to examine the cornea. Therefore, I hold that in most cases of adult ophthalmic gonorrhœa there are already existing abrasions when the cases came under treatment, rendering the prognosis unfavourable.

On the other hand, the infant, a few days old, cannot put its hands up to its eye to rub it; thus the more favourable prognosis and condition arise. Of course an anxious mother can do, and often does, much to nullify this advantage by smearing saliva, or some other substance, roughly over the eye with the rather remote chance of not injuring the cornea. The indifferent mother is much safer; at any rate, we usually get the cornea undamaged and hence a more favourable issue.

When one considers the circumstances under which the examination and frequent cleansing of the eyes takes place, we wonder that so many corneæ escape. I am referring to infants. The child invariably screws up the eyes and moves its head, which is supposed to be steadied by the nurse. A view of the cornea, on account of the screwed-up eyes and swollen lids, is impossible without the use of a retractor, such as Desmarre's. How very easy, therefore, under these circumstances and in spite of delicate manipulation, to cause an abrasion. Another danger arises in these frequent cleansings. Often nozzles are introduced—flat ones have been specially constructed. The nurse has mostly to do this alone. How is she to steady the head and use this instrument without danger to the cornea every time? How easy it is at the same time to press on and to rub the eye; the circumstances render it almost impossible to avoid so doing. To my mind these frequent thorough cleansings are unnecessary. The secretion can be let out, if one wants to, by opening the eyes in the way I have described and further cleansing it by gently squeezing swabs saturated with boric solution or sterilised water; dried secretion can be dealt with by spreading vaseline on lint and putting it on the eye, and later on gently sponging it off without touching the eye. If an irrigator is used it can be manipulated as I have indicated above. In two of the cases above quoted I used argyrol, chiefly, as I said, for its soothing quality. The case treated with boric solution and later distilled water actually needed a week less treatment than the others.

We have modified our views of energetic treatment in other diseases. For instance, I read in the *British Medical Journal* of July 16 (epitome of current medical litreature) that in Germany many obstetricians were carefully avoiding active local measures in the treatment of cases of puerperal infection by not removing retained pieces of ovum or placenta, abandoning even vaginal douches for fear of doing harm. Again, in our ophthalmic practice how different is the modern treatment of disease of the lachrymal canal! Formerly the duct was slit as a matter of routine, then probes were passed down the passage of gradually increasing calibre, some not satisfied until an instrument the size of a penholder could be passed and a gust of air could be blown through the passage; whereas now we shrink from using the probe except in the gentlest manner and unless we are unable to succeed otherwise, whilst trying by gentler means to coax the duct back to its normal state and function.

These deductions may be founded upon an insufficient number of data; my information has not been extensive, and I cannot hope to revolutionize treatment on the result of three successful cases. Still, the facts are sufficiently remarkable to be worthy of report, especially those of my friend whose paper I have quoted, and whose observations are unassailable, and I think the conclusions are correct. The treatment I advocate is that of non-interference. Usually if one meets with these cases one has to do something, for if treated at home the relations expect it. In the hospital the mother usually accompanies the child and relies upon its being attended to. Even the hospital staff wants to do something. I have attempted to indicate how this may be done with the least possible harmful interference.

IV.—FRONTAL SINUS DISEASE.

By J. Luckhoff, M.D. (Edin.), Cape Town.

(*In the first part of the paper the symptoms and diagnosis of frontal sinus disease were discussed.*)

Case I.—A young fellow came to me complaining of severe pain and tenderness over his left eyeball and over the inner angle of the orbit. He was sleepless and seemed in distress. The trouble he definitely associated with an attack of influenza which had laid him up for two weeks. For a week previous to seeing me he had a dull frontal headache. Three days ago the severe pain started. He referred the source of mischief to the eye, and consulted with reference to that organ.

From the symptoms I diagnosed acute frontal sinus inflammation. After cocainisation and probing thick pus flowed out from under the anterior end of the middle turbinate. Twice a day I washed out his frontal sinus and continued this for a period of three weeks. A sterile warm saline solution was used. Within three days his pain and discomfort had subsided. Subsequently I washed the sinus out twice a week. At the end of ten weeks he was completely cured.

In this case his severe symptoms pointed to threatening complication.

In Prof. Killian's clinic at Freibourg acute frontal sinus inflammations are treated by means of lavage and dry electric heat. Incandescent electric lamps are placed in a wooden box which fits over the head of the patient. Generally a quarter of an hour of this treatment is employed daily. It produces profuse perspiration from the scalp and face. Spontaneous cure of acute frontal sinus inflammation is not uncommon. Free intranasal drainage is essential.

Case II.—A young lady came to me complaining of a dull, almost continuous, frontal headache. It had led to a frown in the skin of the forehead, caused partly by raising the eyebrow, and also by contraction which produced a vertical fold in the middle line. This kind of frown is not infrequent in these cases. Her headaches sometimes became very severe and made life a burden, notwithstanding a naturally bright and optimistic temperament. Nine months before she had had influenza, which was followed by nasal stuffiness and produced a discharge from both sides. Some weeks later her medical attendant removed polypi. The nasal spaces on inspection presented a condition almost identical with that produced by atrophic rhinitis. Dry purulent crusts lay over atrophied turbinate bodies; the odour was not marked. In the nasopharynx similar dry secretion could be seen. Exploration and washing out of the maxillary sinuses gave a negative result; first on the one side and then on the other, I opened up large anterior ethmoidal cells that contained pus. Nine months later, after regular periods of washing out of the frontal sinuses which failed to produce sufficient results, I opened up the left frontal sinus through an incision over the eyebrow. As much bone was removed from the anterior wall as would admit of enlargement of the fronto-nasal duct and the insertion of a rubber drainage-tube. This I anchored to the skin surface. The wound was closed. At a later period the opposite sinus was treated in a similar manner. Subsequent treatment, after removal of the drainage-tube, consisted in washing out with saline and four per cent. zinc sulphate solutions, done intranasally. A year later I re-opened one of the sinuses, as it had not ceased to discharge and still produced headaches. The patient after this recovered completely. No symptoms now remain. The depression left is not much, though it shows, owing to the low bridge of her nose.

Notwithstanding that there was nasal obstruction and fluid pus at first, this case later simulated atrophic rhinitis. Differentiation between accessory sinus suppuration and atrophic rhinitis may be difficult, and not infrequently what at first is looked upon as atrophic rhinitis later on proves to be accessory sinus suppuration, ethmoidal or frontal. To Grunwald we owe the increased caution that is now exercised in the determination of this differential diagnosis.

In South Africa a purulent discharge desiccates sooner than in Europe. We thus frequently meet with symptoms termed ozœna, which are due to sinus suppuration. This case demonstrates that a modified removal of the anterior wall of the frontal sinus may be sufficient to lead to cure, only the after-treatment is more tedious.

Case III.—A young adult, a baker by trade, of Polish nationality and Bantu in his method of getting rid of muco-pus from the nose, consulted me because of fœtid nasal suppuration very copious in amount. From the left nasal space I removed large polypi bathed in pus; these sprung from the anterior ethmoidal region. The maxillary sinus on that side was full of pus. This I opened up intranasally through the inferior meatus. Much pus still came from the left frontal sinus. On this I operated by Killian's method.

In radical operations on the frontal sinus, Killian's method holds first place. A skin incision from over the frontal process of the superior maxilla, passing to the inner end of the eyebrow, then along the orbital ridge to the outer end of the eyebrow, is made to sever soft tissues only. The periosteal incision is carried over the same area, only at a somewhat higher level above the orbital ridge. At the inner part of the orbital ridge the periosteum is not injured. The object of this is to leave a well-nourished border of bone immediately beneath the level of the eyebrow. All the anterior wall of the frontal sinus, excepting for this margin, is removed. As all the floor of the sinus is removed as well, opening thus on to the orbital tissues, a bridge of bone is left along the orbital margin. The object of the downward extension of the incision to the inner side of the nose on to the process of the superior maxilla is in order to open up the anterior ethmoidal cells by chiselling away this process of bone. No better access to the ethmoidal cells than this can be obtained. By this procedure the communication between the sinus and the nose can be made as free as possible. A tube is left to drain the lowest and innermost part of the bone cavity in the forehead; this passes into the nose, and being anchored to the skin of the forehead, remains in place. At the end of two or three weeks it is removed intranasally. All the skin wound is closed. The most dependant part of the incision may suitably be drained for the first three days. Very free drainage into the nose is imperative. The danger area is the innermost part of the floor, where it borders on the cribriform plate. The floor of the sinus can be removed either by entering below the orbital ridge from the front, or by removing it from above through the space made anteriorly. The pulley of the superior oblique, even if disturbed, leads only to temporary diplopia. The supraorbital branch of the fifth nerve is severed. As the purpose of this operation is to obliterate the sinus while still leaving the orbital ridge, a sinking in of the soft tissues occurs in the forehead and also a little at the side of the nose. While ensuring a maximum amount of obliteration with a minimum amount of deformity, this operation possesses advantage over less radical operations which often are insufficient, and over such an operation as complete removal of all the anterior wall and all the floor of the sinus, without retaining an orbital ridge, as this leads to very obvious deformity. In this case the sinus was very large, both in its extension upwards over the forehead and in its extension backwards, which covered two-thirds of the whole orbital roof. In addition it communicated freely, by means of a large aperture in the septum between the sinuses, with the opposite sinus. That sinus also contained much pus. The obliteration of the operated cavity, and its healing process, were very satisfactory. The suppuration in the opposite sinus discharged partly through the intranasal drainage provided on the side operated upon. From lack of opportunity and time the patient was unable to have the opposite side operated upon.

Of interest in this case, we have not alone the enormous size of the cavity and its communication with the opposite sinus, but there was also the obvious fœtor of the fluid discharge prior to the operation.

The necessity of operation on the opposite sinus in this case was characteristically confirmed in the case in which I will next mention.

Case IV.—A lady, in apparently good health otherwise, consulted me for severe frontal headache. Her nasal spaces were much obstructed by an S-shaped bend in the septum. Small polypi were present in the left side, and a good deal of pus discharged into that side. Pus was also obvious on the right side. On several occasions she had more severe headache, and in addition to local tenderness, slight œdema of the left upper lid and over the forehead occurred. This was transitory. Notwithstanding the purulent discharge, I felt justified by previous experience in removing all the deviated cartilaginous and osseous septum between its enveloping soft tissues, leaving these flaps intact. This so-called submucous septal resection was successfully done, healing occurred rapidly and normally, and free access to the left middle turbinate body was now possible. Its anterior end I removed. Notwithstanding daily irrigation of the left frontal sinus for three weeks through its duct, using saline solution for that purpose, only evanescent improvement occurred. Owing to another attack of tenderness and œdema of the tissues on the left side over the affected sinus, I operated by Killian's method. The cavity was filled with dark hæmorrhagic polypi. A free opening existed between the two sinuses. After obliterating the mucous membrane of the left sinus I was enabled to curette the opposite cavity through the mesial opening. It also contained pus-secreting granulation tissue. I then applied pure carbolic to all the surface of that sinus, and hoped thus to obviate operative measures on that cavity.

The intranasal tube on the operated side I left in for three weeks. Speedy healing and good cicatrization of anterior to posterior wall occurred. Ten weeks after the operation she went to her home in the Karoo. A month later she was again in distress and came back to town. Œdema and a tense swelling was present over the operated side. After intranasal treatment for five days the swelling had practically disappeared. The history easily explained itself. The unoperated sinus not finding sufficient exit for its secretion through either its own duct or that of the operated sinus thus accumulated and produced pressure and inflammatory symptoms. I therefore operated on that sinus in a manner identical with that employed on the other one. All went well now. Practically no deformity exists on the side last operated upon, and since the second operation the depression left after the obliteration of the first sinus is much less obvious. No more secretion comes from either side, and the patient has improved remarkably in health and happiness. She no longer has headaches. As to deformity, she is satisfied to have nothing done to improve the depression on the left side. As I corrected her astigmatism too, the spectacles help to distract attention from the defects.

In the previous case a radical operation was necessary on account of a copious fœtid discharge which interfered with the patient's occupation. In this case the operation was called for on account of headache partly, but chiefly because of symptoms that became urgent, such as œdema and swelling. In the case which I will next detail operation was required because of perforation of the floor of the sinus leading to abscess and a discharging sinus.

Case V.—An adult was sent to me by his medical attendant. His history was as follows:—Influenza had lead to acute distress, in the form of pain and tenderness over the left frontal sinus, with much œdema and swelling. Finally an abscess formed and ruptured above the upper lid, beneath the orbital ridge. The discharging sinus failed to heal. Pus was also observed in his left nasal space. On several occasions his medical attendant curetted the sinus. Some months elapsed before he was sent to me. I then found that the sinus led backwards and somewhat inwards along the orbital roof for two inches. That frontal sinus suppuration had led to this condition was undoubted. As there were no acute symptoms I contented myself at first with an intranasal operation. The anterior end of the middle turbinate bone was bulbous and unhealthy in appearance. In removing this portion a large turbinate cell full of pus was opened up. Another large pus-containing anterior ethmoidal cell was next opened up. Notwithstanding treatment through the nose and through the external wound under the orbital ridge, which was carried out for several weeks, no satisfactory improvement was obtained. A more radical operation was then carried out. The usual skin incision for Killian's operation was made. The frontal sinus that lay exposed when part of its anterior wall had been removed was a large one. What first produced considerable confusion and led to much caution was the total absence of the floor of the sinus. This absence no doubt was primarily due to the disease, and partly to the subsequent treatment. The ethmoidal cells were entered in the usual method of Killian. Owing to having left a rather thick orbital ridge, subsequent trouble in the drainage into the nose was experienced. The upper part of the duct occluded a month after removal of the intranasal tube. This necessitated another operation, in which I chiselled away the upper border and posterior surface of the bridge at its inner end. The tube was left in for six weeks after this. Not only is there perfect cessation of discharge, but also the deformity is practically unobservable.

Here again modification of Killian's method led to subsequent trouble, but in removing a minimum amount of the anterior wall a great advantage in appearance was subsequently gained.

Apart from the perforation through the floor of the sinus and the absorption of much of it, it is of interest to note the associated involvement of the anterior ethmoidal and turbinate cells. This is not an uncommon association.

Case VI.—The last case I will record is one also with complications, only of more serious and acute degree.

I was asked to see and treat the patient when he was about to be operated upon. Great œdema and swelling occluded the left eye, and produced a glossy rotundity over the whole forehead. There was a temperature of 103° F., and much pain and sleeplessness. Acute symptoms had started a week previously. A frontal sinus suppuration with acute exacerbation and possibly a periosteal abscess was diagnosed. I operated after Killian's method. The incision at once opened up a large periosteal abscess above the orbital ridge, situated over the outer third of the forehead.

No osseous defect could be found. The usual Killian operation was done. As soon as an opening was accomplished, pus continued to well out under pressure. The granulation tissue present must have been of some months' duration. In smoothing off the front table of the frontal bone at the boundary of the septum, my chisel shaved a little bit off the mesial septum, exposing but not lacerating the mucous membrane of the opposite sinus. After the usual removal of the ethmoidal cells from the facial route, I passed a rubber tube from the frontal sinus into the nasal cavity. It did not slip in smoothly.

Five days after the operation I found it necessary to remove all stitches and to fully expose the wound cavity for purposes of irrigation and drainage; not only was the discharge profuse, but besides being muco-purulent, it was blood-stained and had an odour. Two facts became obvious: there had been further extension of the bone disease, and as there was no mucous membrane left in the operated sinus, the mucoid nature of the discharge and probably its copiousness was due to infection of the opposite sinus and to rupture of the mucous membrane of the middle partition that I had exposed accidentally. Two factors were responsible; both my failure to make the cavity of the periosteal abscess perfectly aseptic, and also an obstructed intra-nasal drainage. In order to gain better conditions, he was now irrigated two-hourly with sterile saline solution through the wound, chiefly the mesial septum. I touched up the suspected areas of bone with argyrol 40 per cent., and daily irrigated the opposite sinus with zinc sulphate, 20 grains to the ounce. Under this treatment matters had improved very much at the end of ten days. He was now submitted to further operation.

I shaved off the upper border of the bridge, carefully curetted the mesial border of the frontal bone, and chiselled away flakes of bone from the outer temporal angle of the cavity, which also showed some affection. After wiping out the opposite sinus with silver nitrate solution, 20 grains to the ounce, I touched up all the borders of the frontal bone with pure carbolic. The tube into the nose had kinked over the upper inner angle of the maxillary sinus. For another two weeks the wound was kept open and irrigated with saline solution—the opposite sinus was daily washed out with zinc sulphate solution. He was now improving rapidly, so I submitted him finally to an intranasal operation on the maxillary sinus of that side. It was full of pus. I now felt justified in closing the skin wound. The intranasal tube I left in for six weeks. Healing occurred perfectly, and the soft tissues approximated and cicatrised satisfactorily to the posterior wall of the sinus. The fact of having kept the wound open for so long made but little difference to the character of the scar in the skin. There is a considerable degree of depression in that side of the forehead. Very satisfactory in this case is the complete subsidence and cure of the purulent infection of the opposite sinus.

What added greatly to the suffering of the patient was the unavoidable contact of the lower lip of the wound during the irrigation with the cut trunk of the supra-orbital branch of the fifth nerve. The irrigation was done by means of a silver Eustachian catheter. Contact with the nerve caused agonising shooting pain, which was referred to the scalp of the vertex.

After the lapse of three months there is still a very little muco-purulent discharge from the maxillary sinus.

The etiology in this case is probably correctly judged as being primarily due to maxillary sinus suppuration, which was exacerbated through his having such very narrow nasal spaces. Infection of the left frontal sinus, probably chronic, was followed by complete blocking of its exit into the nose. The imprisoned muco-pus led to an extensive periosteal abscess externally. The risks in such a case where no line of defence had yet been established were very much greater than in the case (No. 5) previously mentioned. Added to this was the insufficient drainage after the operation. Necessarily in such a case to avoid that dreaded disaster, osteomyelitis, very free drainage and radical measures must at once be adopted.

The fact of leaving the maxillary sinus for treatment at a later date was due partly to the urgency of the operation in the first instance, which did not allow of a complete diagnosis, but also to the duration of the operation and the state of the patient. The infection of the opposite sinus came as a complete surprise to me, and no doubt was made the more possible because of the accidental exposure of the mucous membrane. Perhaps most of all to be learnt from this case is the necessity of perfectly free drainage. Should retention of fluid be detected after such an operation, the wound cavity must be freely opened up. Purulent osteomyelitis leading to a fatal issue is a not uncommon disaster in operations on the frontal sinus; such a danger is imminent when, through retention, pus or other infective fluid is brought into constant contact with the freshly-exposed cancellous tissue of the frontal bone.

The indications for frontal sinus operation were imperative in this series of cases. Apart from the risks incurred during such an operation, there has to be considered the amount of deformity that will remain. It is especially the deformity which may result that induces patients to endure their discomfort rather than be operated upon. It is because of this fact that other means of operation have been sought for. Intranasal operation on the frontal sinus has its advocates chiefly in America, but also elsewhere. Apart from removal of the anterior ethmoidal cells and of the anterior part of the middle turbinate, I have no experience of these methods. An electromotor burr is probably the best method of opening up the duct. A guide is first passed, and the operator works only anterior and external to this. The method is, however, full of risk, and is probably less satisfactory in results than a modified external operation in which the minimum amount of anterior sinus wall is removed.

An osteoplastic flap made out of the anterior wall is very feasible in a large sinus. It is a favourite method among a few operators. But besides being more tedious, this method more readily leads to complications than does the removal of the bone. Any advantage gained in the diminution of degree of deformity is comparatively a small one when balanced against its disadvantages. Though an electromotor burr is of distinct service in the radical sinus operation, both in working on the bridge which we leave and in dealing with the anterior table of the frontal bone, the chisel is the more commendable instrument.

V.—THE OPERATIVE TREATMENT OF SQUINT.

By I. Haslem, Cape Town.

When asked to read a paper to the Congress, I selected this subject, because I felt that it is one about which we have lately to a great extent found reason to remodel our ideas and methods of treatment.

It is in the treatment of our young patients that we take the greatest responsibility, and amongst them monolateral convergent and accidentally alternating squint hold the most important place.

As to the cause, Donder's accommodation theory undoubtedly was nearest the mark before Worth's researches were published. Now I think we can agree that a deficient, or retarded, development of the fusion faculty is the deciding factor as a rule.

Only a few years ago the usual practice amongst ophthalmic surgeons used to be to put some atropine into the child's eyes and prescribe a pair of glasses. If this resulted in the eyes becoming "straight," good and well; if not, an operation was considered the only remedy, and the parents of a young child were advised to wait till the child would be at least 10 or 12 years old before having it operated upon. The result was then that the eye in most cases became hopelessly blind.

In such cases we now, following Worth's method, after giving glasses, atropise the fixing eye only, or bandage it where required, until we have succeeded in making the child use the squinting eye as readily as the other one. The vision is then generally improved from a condition of amblyopia to near normal and the squint is alternating. This may take several months. Then we begin fusion training with the amblyoscope. The one I use is a modification of Worth's, in which I can use pictures 3 inches square instead of 1½ inches. This treatment is only suitable for children under 6 years of age, and where the squint has not produced permanent blindness.

In this way we reduce the number of cases we need to operate on, and likewise the number of blind eyes. We get good binocular vision and perfect sight where we formerly only got an improvement in appearance, or very little more in exceptionally favourable cases, by a late operation.

If a case of squint with good fusion tendency can be reduced to a certain angle, the patient will suddenly "drop" the squint, which will become a latent squint, a heterophoria, in this case an esophoria. This may also ultimately disappear.

In the same way the more or less dormant or defective fusion tendency also accounts for some of our success in squint operations, because it is an absolute impossibility for a surgeon to secure a mathematically correct result. His little error is rectified by muscular action in the interests of fusion, where the tendency exists.

The gradual reduction of the angle of squint under treatment indicates a readjustment in the length of the opposing muscles. The sudden disappearance ("drop") of the squint is produced by muscular action in the interests of binocular vision, and is generally followed by a further and complete readjustment of the length of the muscles. While Worth lays special emphasis upon fusion training with the amblyoscope, and ascribes the good results almost entirely to this, it cannot escape our notice that the atropine-optical treatment plays an extremely important part.

But the amblyoscope is an ingenious device for bringing the two pictures right in front of each eye, developing the child's power of observation, awakening the desire for fusion, training its amplitude, and thus greatly assisting in securing a good result.

Cases requiring operation have thus been materially reduced in number, but the stronger light thrown upon the danger of amblyopia ex anopsia makes it our duty to urge immediate operation where we see no prospect of complete success with atropine-optical treatment and fusion training.

It is no use letting the case alone, hoping that the child may grow out of its squint, if at the same time it grows out of its chance of retaining vision in the squinting eye. It is useless to expect parents to maintain an even oculo-motoric balance in the child's eyes for years, particularly if an intelligent use of atropine or occasional bandaging is required; a relapse into monolateral squint and amblyopia is far too likely a result with a young child. In essentially alternating and neuropathic divergent squint, usually not amenable to optical treatment, it becomes a question of operating for cosmetic purposes. There is hardly ever any fusion sense. I think the right time for this operation is just before the child begins to go to school. It has a depressing influence upon a child to be exposed to the gibes of the other children, and may have an undesirable effect on the development of the child's character. It is possible that in the future a secondary operation may become necessary, but I consider this possibility a lesser evil, and not so likely to occur if the first operation was a properly performed advancement with or without tenotomy of the opponent muscle.

A slight degree of essentially alternating squint, a true occasional squint and infantile spurious monetary squint require no treatment. They are all liable to disappear. But premonitory occasional squint might be mistaken for the two latter conditions, and these therefore require very careful investigation.

Until lately myopic divergent squint used to be considered curable by operation only. This at first appears rather strange, because amongst the number of myopes treated with glasses one would expect that *some* cases would have been observed where the squint was cured by wearing glasses. But the explanation probably lies in this: Myopia is developed later in a child's life, and glasses are generally given *before* the squint develops. There has always been a tendency to under-correct myopia, especially for near-hand work. The opposite condition to the one existing in uncorrected hypermetropia is then allowed. Little or no effort at accommodation is associated with little or no effort at convergence; if diplopia results, the child turns the eye further out. Converging to a very near point—especially without accommodative effort—also is very tiring and difficult to keep up. It is perhaps after all not so strange that we did not find that the wearing of glasses cured myopic divergent squint. The motor balance in these cases needs to be studied before prescribing glasses acting as prisms with the base in, or we may foster the development of a divergence. But they may be required for the patient's comfort, or an operation may be necessary for the same purpose as well as for binocular and cosmetic purposes.

Neuropathic divergent squint generally requires operation, and the result is satisfactory in proportion to the acuity of vision in cases of amblyopia, the sense of diplopia and fusion faculty, where there happens to be some, and the power of dynamic convergence in association with accommodation. Unfortunately these factors are very often not in our favour, and we have to be satisfied with putting the eyes into a medium position, from which there may be an occasional convergence as well as divergence. This applies to cases of essentially alternating convergent squint as well. There is a curious similarity—or shall we say, relationship?—throughout between these two conditions, and the differences, where they are found, may be explained as more or less accidental. I show two recently operated cases illustrative of these two conditions. We sometimes have to deal with conditons which used to be called latent squints. They used to be looked upon as differing from manifest squints only in this, that the patient by an extra effort of the feebler set of muscles overcame the tendency to squint, whereas the essential difference is that they have a good fusion faculty, and cannot suppress the sense of diplopia when one eye is misdirected.

Esophoria, exophoria and hyperphoria of high degrees usually require operative treatment. In hyperphoria the trouble is usually due to preponderance of the superior rectus, and tenotomy of this muscle should then be our choice. I look upon the internal rectus as a more powerfully acting muscle than the external rectus. In exophoria and divergent squint, therefore, we have in a weaker external a preponderance in action over what should have been a more effective internal rectus muscle. Clearly in that case the remedy is not to weaken the externus by a tenotomy. The result would simply be a reduced efficiency of both.

By advancement of the internus, however, we increase the efficiency of both. What is the condition, when an eye has been put straight by an advancement? The advanced muscle is not as efficient as the same muscle in an unoperated straight eye. A certain weakness of action existed before the operation; the efficiency of the muscle presumably suffers some further loss of efficiency as a result of the operation. But the *oculo-motoric balance is now maintained by the unoperated muscle taking over some of the work which before fell to the share of the operated muscle!*

This is the essence of the success of advancement operations and the explanation of the many more or less unsatisfactory results after more or less extensive tenotomies. The sufficiency of a tenotomised internus is after the operation subject to a further gradual loss, and the slackened externus gradually becomes shortened and likely to become excessive in its action. For reasons which I will go into later on, the same does not apply to tenotomy of the externus. Where tenotomy is made in conjunction with advancement, the connections between the muscle and the eyeball are never so extensively severed as where one relies entirely on tenotomy, and the severed tendon becomes fixed in a position to which it is stretched by the tension of the advanced muscle, instead of where it would retract to in simple tenotomy. This makes an enormous difference to the result.

My idea that one rectus muscle can take over part of the work of its opponent may seem somewhat self-contradictory to you, but you must remember in this connection that it is a matter of indifference to the muscles where the visual axis happens to find itself after the operation; that whatever fusion sense or consciousness of diplopia an operated patient may have, it was not connected with the pre-operative "muscle sense," and will readily make the best of the new position of the eyes, provided this be approximately correct.

I exhibit a diagram to illustrate how the opponent takes over part of the operated muscle's action. Say we have a convergent squint of 30°, and that for optical purposes we require a lateral rotary action of 60° to right and left—120° in all. To produce this before the operation, the supposedly stronger rectus internus has to rotate the eye from its position of balance by active contraction over an arc of 30° of the circle, while the "weaker" rectus externus, to produce the corresponding movement to the temporal side from the position of balance, has to rotate the eye over an arc of 90°, or three times as far as the "stronger" muscle. After the operation they will both be called upon to produce a rotation of 60°—the rectus internus has taken over 30° of rotation which previously fell to the lot of the rectus externus. (I use the word "active" contraction, etc., without prejudice to any question about a more or less "passive" return to the position of balance.)

Regarding the muscles in this light also accounts for the apparent aphorism that the predominating muscle sometimes is found to have insufficiency of action, a natural result of its reduced activity. It also accounts for the small result of certain tenotomies. If the shorter muscle is the less efficient one, "cutting it" is not going to push the eye much over in the direction of the elongated muscle, nor is the slackening of the two going to increase their rotary efficiency. We are liable to deplore insufficiency of dynamic convergence after a tenotomy of the interni, or an ineffectual operation, especially in the case of external tenotomy, or a combination of both deficiencies with asthenopic symptoms and perhaps diplopia thrown in.

You may be amused to be reminded that the prehistoric position of our eyes was one of divergence. Still, this aspect deserves to be considered in all seriousness, because it helps to explain the different results of corresponding operations for convergent and divergent squint. Binocular vision is a late evolutionary acquirement, in the interest of which our present "straight" position of the eyes is developed and maintained in spite of certain difficulties. In the early embryonic stages our eyes diverge like those of the fishes. That the acquired parallelism is not yet at the present stage of our development absolutely stable, and that there is a rudimentary tendency to divergence, may be concluded from the direction of the eyes in cases of total blindness, total paralysis, general anæsthesia, and death.

In cases of congenital paralysis of the abducens the internal rectus suffices to keep the eye in its proper position, except when the glance is directed towards the side of the paralysed muscle. The forward direction of our eyes is dependent upon a developed preponderance of the rectus internus over the rectus externus. This aspect, taken in conjunction with the anatomical relations, which do not favour the reinsertion of the rectus internus to the same extent as the rectus externus, in my opinion accounts for the difference in results referred to. The disadvantages of simple tenotomy become considerably reduced where we have to deal with a "stronger" (or shorter) muscle which has an excess of sufficiency, and where some of the lateral fibres may be left undivided.

In esophoria with insufficiency of the externi we make the interni take over some of the work of the externi by advancing the latter, whereas if we find normal or hypernormal dynamic convergence and fairly normal sufficiency of the externi, we may perform tenotomy on the interni, if the tendency to convergence corresponds to or exceeds the expected result of the operation. In a case of convergent squint where one does not wish to alter the direction of the eye more than 10° to 13° for each eye, where the dynamic convergence is good and the externi sufficiently active, one may fairly safely perform tenotomy, but if circumstances permit it, I always prefer to rely chiefly on advancement of the externi.

With regard to the ultimate result of squint operations, there used to be a great deal of variety of opinion. One seemed to find no fixed rules to go by; sometimes the effect of the operation increased afterwards, sometimes it decreased, and nobody seemed to know or agree about which result might be expected. It was known that tenotomies on the external rectus were often disappointing, but with regard to the other operations, the uncertainty of the result held out, the chaos of conflicting statements, was enough to produce despondency in the most cheerful operator. I think the reasons for this unfortunate state of affairs were that the essential conditions leading to squint and complications resulting from it, the insufficiency, etc., were not taken into consideration as guides for our *modus operandi.*

On the side towards which the eye squinted there was a muscle which was too strong and active, and so we cut it in two and left it to itself to find its proper sphere of action. If a thorough-going double tenotomy was not sufficient, we made a slight advancement to make up the deficiency, sometimes by hitching up the opponent with a couple of stitches. There is one other little matter that has been overlooked. I mentioned before that we must expect some further impairment to take place in a muscle that has been operated upon, a loss of dynamic efficiency. Supposing, then, that I am right in saying that the internus is stronger than the externus, and we perform an advancement with tenotomy. Both muscles are liable to, say, a loss of 20 per cent. of their efficiency; 20 per cent. of the internus is more than 20 per cent. of the externus. We should then get a dynamic alteration in favour of divergence if at the time of the operation we put the eyes "straight." This is exactly the tendency we find. It is therefore advisable, especially when operating for high degrees of squint, to slightly under-correct convergence and over-correct divergence. In this country the overwhelming majority of squint operations consists in simple tenotomy of the internus. Unfortunately that will probably remain so for some time to come, as it is such a simple operation.

I therefore conclude my paper with a short description of the method I follow when I have a case suitable for this operation. A small horizontal buttonhole is made through the conjunctiva and capsule of Tenon at the upper edge of the insertion of the tendon, just large enough to introduce my hook and scissors. The hook is inserted under the tendon, the point turned forward below the lower edge, and the tendon cut between the hook and the eyeball. No suture is needed; there will be no sinking in of the caruncle; the wound is covered by the upper eyelid. If the eye is kept clean, no bandage is required.

VI.—ANGEIOMA RETINÆ: NOTES ON A FEW CASES.

By D. J. Wood, M.B., C.M. (Edin.), Cape Town.

(Not supplied for publication.)

F.—SECTION OF PATHOLOGY.

I.—THE INCIDENCE OF CANCER IN SOUTH AFRICA.

By G. A. Casalis, M.B.

(*Special Subject for Discussion.*)

I may say at once that, in the following remarks, I shall eschew any reference to the domain of either pathology or bacteriology, at least in so far as these sciences are related to cancer, first and foremost because it is not in my province to do so, even although the agenda of the section has included the characteristics of cancer as one of the points for discussion. I am not aware that they are different in this subcontinent to what they are elsewhere. Secondly, because, as pointed out by W. Roger Williams, " the terrible welter of disjointed facts and contradictory hypotheses that constitutes such a large part of modern tumour science " makes it impossible, in a very rapid and superficial sketch such as this is, " to reduce into a semblance of order the chaotic mass of material already accumulated, or to deduce from them such conclusions as the facts seem to warrant."

As regards incidence, however, especially as referring to this country, the simplest and most casual of investigations is at once rendered valueless by the practically total absence not only of reliable statistics, but even of any statistics at all. Gentlemen, it is a deplorable fact that on such an intensely interesting subject, and one which at the present day looms so largely on the medical horizon, we have so far not compiled any statistics of importance on malignant diseases. Except for a few statistical notes I have obtained from the Cape Colonial Health Office through the courtesy of Drs. Gregory and Robertson, nothing has been published and nothing is known. This is all the more to be deplored because, as I will point out presently, South Africa would appear to be in a specially favourable position to study the disease. The trend of scientific thought, after being obsessed for years by the microbic theory, is now slowly veering round, and is inclined to deal with the subject of cancer genesis from a natural history point of view rather than from a purely bacteriological and pathological standpoint. Now, I venture to state, nowhere else in the world are the ethnological conditions so admirably adapted for the study of cancer as we have them here. On the one hand we have a huge native population numbering close to 7,200,000, among whom the disease, until quite lately practically unknown, for reasons difficult to define seems now to be on the increase. On the other hand we have a white population of only 1,200,000 souls all told, which show perhaps not such a high rate of incidence as in some parts of Europe, but amongst whom—and I believe most of you will bear me out—cancer is certainly prevalent, and possibly unusually prevalent, taking into consideration the simple form of life led by the majority of sufferers. Further, we have a mixed coloured race holding both to the pure white and to the black and Malay races, where I will show, or attempt to show, even in the absence of all statistics, the incidence of cancer is considerable, and bears no comparison to the immunity found among the pure native races.

Now, why this difference among inhabitants living at the same altitude, under the same climatic conditions, and in some cases practically side by side?

Why, for instance, should the Dutch colonist, settled on the high plains of the Free State, on the conquered territory, suffer from cancer, while among Basutos living just across the Caledon River the disease was until lately practically unknown, and is still so rare as to be somewhat of a pathological curiosity?

In both countries the soil is the same, the rainfall identical; there are no trees to speak of, no marshes; life is eminently a healthy and outdoor one; there are but differences of colour and of mode of living, particularly of *diet,* one being that of the primitive savage, the other that of the European.

To the unbiassed mind it would therefore appear reasonable to assume that if the problem of cancer genesis has a natural solution, it should perforce be found in some of the ætiological factors differentiating the two races alluded to, and must have some relation to either questions of colour, of mode of living, or of civilisation.

Now you all know, at least so far as the cancer problem is concerned, that we have hitherto found ourselves in a particularly unfortunate position of confusion and conflicting ideas, owing to the radically opposite views held by leading authorities as to the ætiology of the disease. There are two conceptions of cellular neoplastic pathogeny, one based on the cell theory, the other on the microbic theory.

The question is: Is cancer a specific disease? Is it an infective one? Will bacteriologists discover its real nature at the end of the microscope; in the incubation tubes of their laboratories? Or is it simply a degeneration, a heterotopic overgrowth of an individual cell, an abortive attempt of certain cells, either owing to certain irritative causes or possibly purely psychic capacities, or, again, simply mnemonic propensities, acquired through successive generations, to repeat agamically some portion of the normal ontogeny? Or, briefly stated, do cancers and other tumours arise through a modification of the formative process, or are they the outcome of an inflammatory process due to the intrusion of microbes or other irritants *ab extra?* In other words, are they essentially of an intrinsic or extrinsic origin?

Those who incline to the former view look to the biological properties of the cells of the affected part for the key of the problem, whereas the other side refer all the essential phenomena of the disease to the activity of parasitic microbes or other external irritants.

Now I have neither the time nor the ability and knowledge necessary to discuss both sides of the question. I will therefore state at once that my firm belief is that both views are partially correct,—both hold part of the truth. I believe that cancers, indeed all neoplasms, although they mainly arise from the abnormal play of forces generated within the body, only do so when the proclivities of some particular cells to heterotopic proliferation have been goaded into activity by some extrinsic causes of which microbes and irritants appear as yet to be the most potent factors.

Along with Roger Williams, I believe that in every healthy organism there is a force which regulates the growth and development of tissues and organs in relation to each other and to the organism as a whole, and that this force maintains the process of all cell growth, multiplication and decay in a state of equilibrium during the whole life of the individual. So long as the growing cells are subject to this normal influence they develop in a regular and orderly manner in accordance with the specific tendency of each indivdual. But when under pathological conditions this restraining influence is modified or withdrawn, then the potential activity to reproduction may become active, and such cells grow and multiply abnormally. As so clearly put by the same author, " hyperplasia and not inflammation is the starting-point of every neoplasm," although clinically we are bound to admit that inflammations apparently in a predisposed organism and one with cells inclined to excessive proliferative activity seem very often to lead to such hyperplastic cell growth.

In spite of what has been said to the contrary, surgons do know that Paget's disease, leucoplasia, chronic ulceration of the stomach, chronic gall-stone disease, cervical erosions, ovarian and myomatous tumours, without speaking of the claypipe, the soot in chimney-sweeps, X-ray dermatitis, or other chronic irritative inflammatory lesions, frequently turn into cancer. Why they should do so we cannot as yet positively state, but that they do is now, I think, proved beyond doubt. To state, however, that cancer, in view of the above, is a microbic disease appears to me hazardous; because, though many of us believe microbes have a preponderating influence in originating such chronic lesions, we do not as yet possess conclusive evidence that they can lead to true cellular neoplastic proliferation. Before a disease can properly be called parasitic the parasite must be found, isolated, and the disease it is alleged to cause must be reproduced by inoculation. Judged by this standard, the microbe of cancer has not yet been discovered, for I am not aware any of the bodies hitherto described as such comply with these requirements.

Of course, the mere fact that no specific infective agent has yet been discovered does not always imply that it is non-existent, nor can the absence of contagion *per se* be regarded as a proof that there is no such thing as a cancer microbe, because we know that some diseases, such as malaria, for instance, although unquestionably of parasitic origin, are not transmissible by ordinary contagion. But where cancerous growths differ so materially from the inflammatory pseudoplasms of microbic origin with which they have been so frequently compared, as, for instance, " Jensen's tumour " in mice, is this: that whereas a great morphological difference always obtains in all malignant tumours according to the locality whence they originally arose, granulomata, on the contrary, no matter in what part of the body they may have sprung, invariably present the identical appearances of all granulomata. What, for instance, can be more strikingly different under the microscope than the appearances presented by sections of growths occurring in the breast, stomach or the skin? And what is more similar than a granulomatous nodule to another? Further, if cancer be purely the result of inflammation and micro-organisms, why should there be such great similarity between primary and secondary growths?

Why for instance should cancer of the breast, produce in the liver metastases typical of mammary carcinoma and not of hepatic cancer? Surely if the secondary lesions be really due to microbes they might be expected to incite the liver cells to malignant proliferation rather than imitate the parent tumour.

Such facts, and others I need not mention at present, are impossible of any valid explanation, unless we admit the presence of emigrant cancer cells which, for reasons unknown, are able in their new home to reproduce their like by growth, multiplication and organisation.

While, therefore, we may confidently assert that we believe chronic inflammatory lesions and irritative influences are frequent causes of cancer incidence—are, as a matter of fact, the *exciting* elements—we cannot as firmly point out what is the nature of the real predisposing causes to cellular neoplastic disease.

These causes, however, as may be said of most predisposing influences, belong not exclusively to the domain of bacteriology or pathology, but mostly to that of biology and natural history. When the issue at stake is thus plainly defined you will see at once how great is the importance of not looking solely for any particular "materies morbi" in any group of cells or specific growth, be they Doyen's *micrococcus neoformans,* protozoa-like bodies, or blastomycetes, for the true genesis of cancer, but, enlarging our point of view beyond the field of microscopy, search for the solution of the problem among those neglected biological factors which, owing to the predominance of the microbic theory, have so far received but scant recognition.

Now looking at the problem of cancer genesis from a biological standpoint and that of tumour growth in general, we are at once struck by the well-known fact that such growths are not the exclusive prerogative of mankind, or even of animals. Tumours are known to arise, under certain conditions, in any multicellular animal or vegetable being. But a no less remarkable observation is that these abnormalities rarely take place in organisms living in a state of nature. It is almost exclusively among domesticated varieties that they are met with. Savages and wild animals are very rarely thus affected; at any rate it is remarkable that nearly all the examples of animal tumours hitherto reported, malignant and otherwise, should have been met with among our common domesticated species, the animals most frequently affected being the dog, the cat, the cockatoo and the mouse, the last an ordinary dweller within our tents and a voracious omnivorous one to boot.

Is it, therefore, not possible,—nay, probable,—that the artificial influence of domestication or other similar abnormal conditions of existence which we impose upon these animals, should have led, as it does in man himself, to a proneness to malignant disease? At any rate, whatever may be the real nature of tumour formation, there is no doubt that the process cannot be regarded, as some bacteriologists would like us to regard it, as an isolated pathological entity having no connection with other biological processes. Between what is known as morphological variation,—a physiological evolution now fully recognised and established,—and tumour growth, there is some affinity, and, as pointed out by Williams, both may well be "the outcome of the cumulative effects of changed conditions of existence." Of these conditions the most important are certainly changed environment and excess of food.

We all know that changed food, changed habits, changed climatic conditions, are potent factors in pathogenesis. Why should they not tend in the direction of tumour growth, as they do in that of the production of other pathological processes?

Another striking feature in connection with the incidence of cancer is that, although the disease has a wide range of distribution and is practically ubiquitous, it has a much greater prevalence in some localities than in others. It is thus frequent among the well-to-do, the highly nourished communities of Occidental Europe, whereas it is practically unknown among uncivilised people as we find them here in South Africa.

Now most of you will doubtless agree that one of the chief physical differences between savage and civilised man consists in the latter being a much better nourished individual than the former, one of the reasons why the native, for instance, is so fond of meat being that he so rarely gets it, or at least not as regularly as the white man. The food supply of the ordinary savage is frequently irregular and precarious, occasional gluts alternating with long spells of enforced abstinence. Further, the food is at times badly cooked and indigestible, whereas that of civilised man is not only always available but highly nutritious and well cooked.

Should the process be reversed, however, and from savagery the black man become civilised, or partly so, specially should his food supply become regular, plentiful and highly nutritious, there are indications which go to prove that he gradually loses the comparative immunity he enjoyed towards malignant diseases, in fact in some cases has a higher death-rate from cancer than the white man.

Take, for instance, the case of the ancestors of the negro slaves of the United States. In their native African homes they were, like all other black races, almost exempt from malignant tumours. Most of the early writers, among whom is Du Chaillu, testify to this comparative immunity. Transplanted to America, however, and having lived there in slavery for over two centuries, these negroes were then found by the United States authorities to have acquired greatly increased liability to cancer, to which, however, they were still very much less prone than their white masters. During the emancipation period and afterwards the proclivity to malignant tumours greatly increased among them, the mortality returns of 1900 showing a death-rate from this cause of 48 per 100,000 living, as against 66.7 for whites. The Louisville City Hospital reports and the data collected by Matas for New Orleans, which there are good reasons for regarding as more reliable in reference to this particular point than the national statistics, show that malignant tumours are now as common there in negroes as in whites. With regard to uterine cancer, negresses are now more prone to it than their white sisters, the respective death-rates for 1900 being 20 for blacks and only 15.7 for whites.

Dr. Watkins Pitchford will kindly note also that in spite of there being no alteration in skin pigmentation, mammary cancer is also now nearly as common in negresses as in white women, the respective death-rates for 1900 being 7.9 for the former and 9.1 for the latter.

Thus, from a condition of almost complete immunity, these negroes have now, according to R. Williams, from whom the statistics are obtained, become nearly as prone to malignant tumours as their white neighbours, and he concludes that this result appears to be due to a violent alteration in their mode of life, with its predominant gluttony and laziness. In like manner the North American Indians are believed to have been almost exempt from cancer in their primitive savage condition; but with the altered mode of life consequent on contact with modern civilisation this comparative immunity has now to a large extent pased away.

From indications of this kind I shall presently attempt to show that among South African natives and coloured races improved alimentation, general comfort and specially altered diet, appear also to favour the incidence of cancer—a disease hitherto unknown among certain Bantu tribes. Thus, my father, the late Dr. E. A. Casalis, who had practised and lived in Basutoland for over twenty-five years, told me he had never, during his whole extensive medical experience, seen one single case of carcinoma occurring in a pure-bred Mosuto. He died eighteen years ago.

In 1890 I was quartered at Thlotsé Heights, Basutoland. I was then in charge of a small native hospital and dispensary. Already I was interested in the cancer problem, and during the four years of my residence in the country I hunted high and low among my numerous patients, numbering several thousands a year, for the disease. I failed to detect one single case. Once I thought I had done so, when a woman with a fungating indurated sore of the breast came for treatment at the dispensary. The growth, on examination, proved to be syphilitic, and rapidly disappeared under the influence of potassium iodide. Several years afterwards my successor, Dr. Macfarlane, was able to give a different report. Thus, in 1900 at the same hospital 3,128 Natives were examined for various diseases, and of these, four had malignant and twenty-five non-malignant tumours. In the same year, of 4,634 patients treated at the Maseru Dispensary, two had cancer and forty-four various non-malignant tumours Of 2,679 Natives treated at the Mafeteng Hospital, six had cancer, four sarcoma, and twenty-two various non-malignant tumours; and at Quthing, of 1,242 patients, two had cancer and seven non-malignant tumours. Thus in one year eighteen cases of malignant disease had occurred in 11,723 patients—a very small percentage it is true, but a marked change from the total absence I had found a few years before, and a still more remarkable one compared to the immunity Basutos enjoyed thirty to forty years before.

I am told by medical officers now resident in the country that they not infrequently see cancer cases, both of the breasts, uterus and other organs, but to what extent I was unable to find. Now it is a well-known fact that the material welfare of the Basuto race has enormously improved within the last fifty years, owing to uninterrupted peace and better government. Food has become plentiful, the natives do not experience alternate periods of famine and of plenty as they did in former years, and further, their diet is more liberal, meat being a frequent article of consumption, although it is certainly not consumed to the extent it is by the Dutch farmers across the border. The continual intercourse of Basuto boys, and even of women, with such highly civilised communities as the Rand and Kimberley has also greatly altered their primitive mode of life. The Mosuto is fastidious, a lover of a variety of tinned preserves, of sugar, sardines, coffee and all sorts of condiments which the pure Zulu, for instance, would not dream of touching. Hence it is that cancer appears to be more prevalent among them than among the latter, where, according to the M.O.H. of Natal, in 1903 out of a population of 86,000 with a general death-rate of 19 per 1,000 only one death was ascribed to cancer. It would be interesting to find how many pure-bred natives are affected by malignant diseases on the Rand, and how far the disease is prevalent among mine boys compared to the house-boys. The latter, as is well known, are much better fed than the former, the mines trying very rightly to keep the native to the staple food he is accustomed to feed upon at his kraal.

In connection with the subject, I note that in the *British Medical Journal* of May, 1906, Dr. H. Goodman of Johannesburg described a case of sarcoma occurring in a Zulu, and personally, in February of this year, I saw Mr. Temple-Mursell operate upon a pure Mozambique boy for adeno-carcinoma of the liver at the Johannesburg Hospital. That meat, condiments, and all highly nutritious albuminous and proteid substances also favour the incidence of cancer among Native tribes or Creoles other than those of South Africa has just been made the subject of an interesting paper by Dr. W. Renner, who, in the *British Medical Journal* of September 3rd, 1910, gives statistical reports of the incidence of the disease among the liberated Africans of Sierra Leone. Contrasting Creoles with pure-bred natives, he says: " The existence of cancer and other malignant growths among Creoles and its absence or rarity among the aborigines are due, in my opinion, to the civilised habits of, and the civilising influences operating upon, the former, and to the primitive mode of living of the latter. The Creoles have adopted the mode of living, the food and dress of Europeans, have to a great extent discarded the simple food of their forefathers, have been craving for and indulging in preserved and imported foreign food, have substituted the European for the natural African environment, and entailed on themselves, in their eager pursuit for wealth and luxury, the anxieties and worries incidental to civilisation and consequent liability to premature decay." Whoever knows the coloured population now so prevalent at the Cape will recognise in the above picture the same conditions which have favoured so markedly the genesis of cancer among African Creoles. That the disease is among them on the increase I have little doubt, although satisfactory statistics are wanting. One practical experience I have gained is that among coloured individuals the incidence of cancer seems to take place at an early age, and consequently is of an exceptional gravity. Some years back I operated upon a coloured woman at the Woodstock Hospital for uterine cancer. The patient was only twenty-four years of age, and the disease had run a particularly rapid course.

In the few statistical notes Dr. Robertson has kindly favoured me with, I find that, whereas among Europeans the age of highest incidence is fifty-five, among coloured people—of whom two are pure blacks,—it is forty-five years. Thus, from March 1st, 1901, to September 30th, 1910, a period of nine years, the deaths from malignant disease in the Cape Peninsula have numbered 1,064, 722 occurring among European, 342 among our coloured population, of whom six cases were known to be Kaffirs. Of these, 1,064 deaths, no less than 434 took place under forty-five years of age—a marked difference to European statistics, where age incidence is highest between sixty-five and seventy-five years of age—the death-rate per million living of each sex for 1900 being highest for males at seventy-five—no less than 6,715 having died of this cause—as against 7,463 females.

The same observation has been made in Natal, for I note that in the report of the Natal Cancer Research Committee the returns, meagre as they are, indicate an exceptional proportion of cases of carcinoma at a relatively early age. There have been fatal cases of cancer of this form at the age of 29 (two), 32 and 34 amongst Europeans, and 20 and 27 amongst Indians: seven fatal cases have been recorded at ages varying from 17 to 30 (*South African Medical Record,* page 254, October 22nd, 1910). As regards the proportion of cases among coloured compared to pure whites, the following numbers of cases admitted in the New Somerset Hospital, and which Dr. Mossop, the Resident Medical Superintendent, has very kindly looked up for me, show how markedly the latter are also affected by malignant diseases. Thus in 1907, 27 European and 14 coloured patients were admitted as suffering from neoplastic growths. In 1908, 50 whites as against 29 coloured; in 1909, 44 whites as against 26 coloured.

If we take the average number of patients treated at the New Somerset Hospital to be 3,000 per annum, we have a total of 190 cases of malignant growths in close to 10,000 patients admitted during three years in the largest Peninsula Hospital. Certainly not an alarming proportion compared to European statistics, but still large enough for our restricted light-coloured and white population.

If we contrast these numbers with those I have given as occurring among Basutos, and still more among Zulus, we shall gauge at a glance the gulf as regards the liability to cancer which separates the pure-bred black from the coloured and the white man. Among the latter a statistical table, unfortunately not yet available, would, I believe, plainly show that between English descendants and Dutch settlers the disease is far more frequent among the latter than the former; nor is the reason for this increase difficult to find, as, although living in a particularly healthy climate, the Dutch colonist, owing to a diet largely composed of meat, coffee, heavily spiced albuminous and other proteid substances, combined with an apathetic and lackadaisical temperament, easily falls a prey to a disease which is favoured above all by defective cellular metabolism. As pointed out by Roger Williams, when excessive quantities of such highly stimulating forms of nutriment are ingested by beings who present the special pathological tendencies described above, I believe there may thus be excited in those parts of the body where vital processses are most active, such excessive and disorderly proliferation as may eventuate in cancer. However this may be, I am persuaded that the few facts I have so imperfectly placed before you justify the belief that there is a certain relation between conditions of existence, in which alimentation plays the leading part, and the incidence of cancer.

To find out what these conditions are would, I believe, go a long way to assist in the solution of a problem which, so far, has baffled the most acute as well as ardent of investigators. As I have already pointed out before, here in South Africa we are placed in particularly auspicious circumstances to study from a biological point of view the incidence of cancer growth; and I trust the day is not far distant when we shall be able to add our quota of observations and deductions to those that are being gathered all the world over on this interesting, but also, owing to its almost always fatal termination, *tragic* disease.

DISCUSSION.

The PRESIDENT (Dr. Watkins-Pitchford) expressed his sense of the exhaustive manner in which the introducer had gone into the subject of causation. They were not going, however, to find out the cause of cancer at that meeting. What was required was organised research and collection of statistics. They might possibly find that greater liability amongst certain people might only apply to cutaneous forms of malignant disease. American statistics should hardly be taken as they stand, because there is a fallacy in the fact that in that country everyone not pure white is classed as negro. The Natal observatons for six years proved that carcinoma of the breast was not nearly so common in black as white. Dr. Casalis's theory of over-nutrition was discounted by the fact that in Natal they did not find that town Natives showed more cancer than those in the kraals, indeed rather the reverse, although the figures were scanty.

DR. TOMORY, confirming Dr. Watkins-Pitchford's remark as to the fallacy in the American statistics, pointed out also that the American negro lives just as well as the white man, indeed very much better than the Italian and many other immigrants. This, perhaps, rather assisted Dr. Casalis's proposition.

DR. DE KORTE pointed out that the influence of nutritional excess should be general, whereas cancer was sharply localised. Non-malignant tumours like ordinary warts were certainly infective, and it might be useful to investigate such things. We would very much like to find out what particular feature of civilisation was a causative factor.

DR. PORTER mentioned that the fact of the Johannesburg natives not being usually of the cancer age rendered statistics about them of no value.

DR. D. DE VOS HUGO said that for years he had never seen cancer in a Colonial-born person till recently, but lately had come across several. Most of the cases he had seen amongst Coloured people of a definite European type. He hazarded as another etiological hypothesis the influence of radio-active bodies given off from cooking utensils.

DR. CASALIS, in reply, apologised for not being a pathologist. We know that cancer was rare amongst Natives, but we want to find out whether it is increasing amongst them or amongst the Coloured. In reply to Dr. de Korte, he thought that the localisation was due to some irritating determinate factor.

II.—PARATYPHOID FEVERS IN SOUTH AFRICA.

By J. G. McNaught, M.D., D.P.H.; Major, R.A.M.C.

When I was asked to prepare a paper for the South African Medical Congress, I found it somewhat difficult to choose a subject. There are many medical problems awaiting solution in this country, but with most of them we in the Army have little opportunity of making more than a superficial acquaintance. Of recent years, thanks to improved sanitary conditions, dysentery and liver abscess have become rare, bilharzia occurs only in isolated cases, and even typhoid fever, though still too common, has fallen to a comparatively low level. The following statistics will show how great has been the improvement:—

Per 1,000 per Annum.		1902-5.	1906.	1907.	1908.
Enteric Fever	Cases	26.4	9.3	10	9.2
	Deaths..	3.02	1	.65	.78
Dysentery ..	Cases	15.2	4.6	4	1.7
	Deaths..	.37	Nil.	.06	.14

The continued fevers still remain the subject of greatest interest to us, and of late the question of paratyphoid fever, its prevalence, its forms, and its relationship to, and differentiation from, true typhoid fever, has attracted a great deal of attention. The subject is one which still requires a great deal of elucidation, and it is more with the view of throwing out suggestions for future lines of investigation than with the hope of contributing very much to its solution that I venture to address you.

It was only in 1896 that bacilli of the "paratyphoid" group were first described. In the first instances they were discovered in urine and in pus; in 1898 they were isolated from the blood of patients. Since that date a great deal of work on the subject has been carried out, chiefly in France and Germany. Before giving a *résumé* of the present state of our knowledge of paratyphoid fever, I shall shortly state the characters of the bacilli concerned. Their properties place them in the coli-typhoid group, intermediate between *B. coli* on the one hand, and *B. typhosus* on the other.

B. paratyphosus, B., the commoner variety, differs from *B. typhosus* in forming acid and gas in glucose media, while it differs from *B. coli* in not forming acid and gas in lactose media; *B. paratyphosus, A.*, differs from *B. paratyphosus, B.*, in forming permanent acidity in litmus milk, while *B. paratyphosus B.*, after a preliminary formation of acid, renders the milk finally alkaline.

As regards mobility, *B. coli* is usually slightly mobile, *B. paratyphosus, B.*, is extremely mobile.

The Gaertner group of bacilli, *i.e.*, *B. enteritidis, B. Danyz, B. surpestifer*, are morphologically and culturally identical with *B. paratyphosus, B.*, and the pathological effects on small laboratory animals are the same. *B. paratyphosus, B.*, however, can be differentiated from them by immunity and agglutination tests.

Paratyphoid fever appears to be much less common in England than in France or Germany. Boycott in 1906 investigated a large number of cases of typhoid fever, and came to the conclusion that three per cent. were "paratyphoid." Savage (L.G.B. Reports, 1908-1909) has more recently carried out very extensive investigations with a view to settle this question, and has come to the same conclusion, *viz.*, that three per cent. of cases regarded as typhoid fever are really due to paratyphoid bacilli. I may add that *B. paratyphosus, A.*, seems to be of extremely rare occurrence; nearly all the cases of paratyphoid fever reported in Europe, at all events, are due to *B. paratyphosus, B.*

	Indol.	Litmus Milk.	Neutral Red Broth.	Glucose.	Lactose.	Mannite.
B. coli.	+ (Exceptionally).	Acid and clot.	Fluorescence.	+	+	+
B. paratyphosus, B.	None or slight.	Acid first, then alkali. No clot.		+	—	+
B. paratyphosus, A.	None or slight.	Acid. No clot.		+	—	+
B. typhosus.	None (rarely slight)	Slight acid (usually permanent).	No change.	A	—	A
B. fæcalis alkaligens.	None or rarely.	Rapid and intense alkali formation.	Yellow colour. No fluorescence.	—	—	—

In India paratyphoid fever is a rare disease among the troops. The A.M.D. Report for 1907 states that out of a very large number of cases of typhoid fever investigated at the Central Research Institute, Kasauli, by means of cultures from blood, urine, and fæces, only six cases were found to be paratyphoid; in four cases *B. paratyphosus, A.*, was the organism present, and in two cases *B. paratyphosus, B.* In 1908 paratyphoid bacilli were only isolated from four cases, the organisms present in three cases being *B. paratyphosus, A.* It would appear that in India, so far as we can judge from the small number of positive results, *B. paratyphosus, A.*, is more frequently the cause of paratyphoid fever than *B. paratyphosus, B.*

In 1897-98 I was stationed at Quetta during a very severe epidemic of enteric fever, and, in collaboration with Lieut.-Colonel Melville, R.A.M.C., applied the agglutination test to a large number of cases, and with hardly any exceptions these cases gave well-marked positive reactions with *B. typhosus*. One case which gave a weak reaction with *B. typhosus*, and whose clinical course was characterised by irregular continued fever and frequent hæmorrhages from the bowel, died. The *post-mortem* showed multiple abscesses of the liver, with ulceration (not dysenteric, nor yet typically enteric) of the large and small intestine. From the spleen I isolated a bacillus intermediate in type between *B. coli* and *B. typhosus* (*Lancet,* February 24th, 1900).

In South Africa the relative proportion of cases is quite different. Clinically, one would expect it to be so, as the number of fevers of indefinite type which it is difficult to diagnose as enteric fever on clinical grounds alone is, in my experience, relatively higher than in India. Major Statham, R.A.M.C., Army Bacteriologist at Pretoria, has devoted a great deal of attention to the diagnosis of continued fevers by means of cultures from the blood. Full particulars of these investigations will be found in the *Journal of the Royal Army Medical Corps* for September, 1907, and December, 1908. He has come to the conclusion that twenty-five per cent. of the continued fevers that come under observation among the troops at Pretoria are paratyphoid fever.

Of the cases of typhoid fever among the garrison of the Cape Colony during 1907-1908, forty-eight in all, ten cases should be regarded as paratyphoid fever according to the results of agglutination tests.

Symptomatology of Paratyphoid Fever.—It may at the outset be stated that it is impossible to differentiate paratyphoid fever from typhoid fever by clinical symptoms. It is true that epidemics of paratyphoid fever may present certain features which broadly differentiate them from epidemics of typhoid fever, but there are no striking characters which enable us to distinguish an individual case from typhoid fever. The following characters have been drawn attention to as specially associated with paratyphoid fever:—

(1) Sudden onset. This also occurs not uncommonly in typhoid fever, especially in hot climates.

(2) A specially profuse eruption. This is also noticed in some cases of typhoid fever.

(3) Frequency of hæmorrhage from the bowel, without the severe symptoms which usually accompany this occurrence in true typhoid fever.

(4) Mild course of the disease. The duration of fever is usually less than in typhoid fever, and the death-rate is low, only some two or three per cent.

Savage records the nature of the eruption in six cases; in four of these, ordinary rose-spots were present; in one case there was also a "rash on the legs," and in one case the spots were larger than those noted in typhoid fever.

Baermann and Eckersdorff (*Centralblatt für Bakteriologie, Referate,* 46 *Bd.,* No. 8, April 18th, 1910) describe a series of eight cases in Sumatra due to *B. paratyphosus, A.* All the cases were fairly severe, the fever lasting from two weeks to two months. No rose-spots were observed; in fifty per cent. of the cases the spleen was enlarged. There were two fatal cases; *post-mortem* examination showed diffuse catarrhal inflammation of the intestines.

Bacteriological Methods of Diagnosis.—From what has been said it will be seen that the diagnosis of paratyphoid fever depends on the use of bacteriological methods. Even with such aids it is not always easy to differentiate these cases. The methods available are (*a*) agglutination tests, (*b*) cultures from the blood, (*c*) cultures from fæces and urine.

As regards agglutination tests, we are met by the difficulty that in some cases a group reaction is present, that is to say, the serum of a patient may agglutinate both *B. typhosus* and *B. paratyphosus B.*, or *B. paratyphosus, A.* Some observers go so far as to say that it is impossible to diagnose paratyphoid fever by serum reactions alone. Cases have been recorded where the serum gave a reaction with one organism, and yet another organism was isolated from the blood or excreta. Most authorities (including Savage) consider that if the agglutination tests are carried out to the limit of highest dilutions obtainable, they can be relied on for diagnosis, the organism which yields the reaction in the highest dilution being considered the causal one. We may, then, have three categories of cases: (1) Where the serum reacts with one organism only, and not with the others; Savage remarks that *B. paratyphosus, B.*, when it gives a positive reaction with the serum, does so in a very sharp fashion, a point which I have also noticed; indefinite reactions are rare; (2) Where the serum reacts with more than one, but more strongly with one than with the other; (3) Where it reacts with more than one, and either equally strongly, or with such slight differences as to be insufficient for diagnosis.

Positive results from blood-culture methods are of more value than agglutination results. It has been abundantly proved that in typhoid fever, if cultures are made from the blood in an early stage of the disease, positive results will be obtained in ninety per cent. of the cases. In the mild indefinite cases which we suspect to be paratyphoid fever, however, we cannot hope to get such a high proportion of successes.

Methods of Obtaining Blood.—(*a*) Finger, (*b*) Vein, (*c*) Plating of clot from samples sent for agglutination tests.

Methods of Culture.—(*a*) Ox bile, (*b*) Ox bile + peptone + glycerine, (*c*) Taurocholate of soda + peptone broth, (*d*) Sterilised water.

From the fluid media after twenty-four hours' incubation at 37° C. plate cultures are made on such media as Endo's agar, nutrose-lactose-litmus-agar, Fawcus's modification of Conrad's brilliant green picric acid agar, etc. Cultures may also be made from the urine and stools.

Epidemiological Considerations.—Some outbreaks of paratyphoid fever have been traced to pollution of water supplies. *B. paratyphosus, B.*, has been found in water; several instances have occurred in this country. Savage believes that infection is usually due to contamination of food, but that no connection can be found between disease in animals and paratyphoid fever in man. Swine fever is due to, or associated with, a closely allied bacillus, but epidemics of swine fever have not been observed to coincide with the occurrence of paratyphoid fever in human beings. Prigge and Sachs-Müke (*Centralblatt*, Referate, Band 46, No. 8, of April 18th, 1910) publish accounts of two epidemics of paratyphoid fever due to food. In one epidemic roast pork was the source of the infection; in the other "kreme schnittchen," baked by a baker who was suffering from paratyphoid fever and passing paratyphoid bacilli in his urine. Contact infection is common, and is contributed to by the fact that only the severest attacks come under medical treatment. Several cases often occur in the same family.

The campaign against typhoid fever now being carried on in the Rhine Provinces of Germany has shown that "carriers" are found in the case of paratyphoid fever as well as in that of true typhoid. While female carriers are much commoner than male in the case of typhoid fever, the reverse appears to hold good for paratyphoid fever. Some investigators have maintained that paratyphoid bacilli are not uncommonly present in the stools of healthy people, and may even be found in their blood as an exceptional occurrence. This view is not generally accepted: Savage's recent investigations for the Local Government Board do not lend it any support.

Prigge and Sachs-Müke (*Klinische Jahrbuch*, Bd. 22, 1909, H. 2) found that 108 persons out of 5,252 examined were excreting paratyphoid bacilli; 38 of the 108 persons were actually ill. They divide the positive cases into two groups:—

(1) Cases which had been in contact with cases of paratyphoid fever, or who had had the disease and had become carriers.

(2) Cases where no connection with paratyphoid fever could be traced.

The two groups of cases can be differentiated from each other by the following characters:—

(*a*) The first group of cases always give a positive agglutination reaction in at least 1-20 dilution; the second do not.

(*b*) The stools of cases belonging to the first group as a rule yield great numbers of colonies of paratyphoid bacilli.

The differentiation of paratyphoid fever from typhoid fever is not a matter of mere bacteriological interest, but has also a practical bearing, *e.g.*, as regards treatment and as regards the estimation of the value of vaccination against typhoid fever. Besides the commonly recognised forms of paratyphoid fever, there are also continued fevers of somewhat similar type, which appear to be associated with other bacilli of the coli-typhoid group. These anomalous fevers are, in my experience, specially common in South Africa, and present such a distinctive clinical type that one is disappointed in not being able, so far as our present knowledge goes, to refer them to one specific organism. In the past three years I have seen three groups of such cases, which I shall briefly describe.

The first group came under notice at Wynberg, and in two cases which were bacteriologically examined *B. fæcalis alkaligenes* was isolated from the patient's blood during the course of the fever. The bacillus agglutinated with the patient's serum. Clinically these cases were very distinctive. The onset was very sudden, with intense headache and photophobia. A characteristic rash appeared on the fifth or sixth day of illness. The rash consisted of dark-red spots and blotches, and came out profusely all over the trunk and limbs, including the palms of the hands and soles of the feet. The rash left brownish stains, which, in one case, took several weeks to disappear The course of the temperature curve was similar to that of enteric fever. Both cases ran a temperature for between two and three weeks, and in both there was a short relapse associated with symptoms of cholecystitis. The serum of these cases failed to give any reaction with *B. typhosus,* but reacted slightly with *B. paratyphosus, B.*

One of these cases was that of an orderly who had been nursing a case of enteric fever which presented certain points of interest. During the course of enteric fever the patient had lung complications, with blood-stained expectoration. On staining the sputum, great numbers of a Gram-negative bacillus were found in it, and on making cultures, *B. coli* was isolated. During convalescence the patient's urine was twice examined for the presence of *B. typhosus; B. typhosus* was not found, but on both occasions *B. fæcalis alkaligenes* was isolated.

Cases of continued fever associated with *B fæcalis alkaligenes* have been reported by several French and German investigators. The second group of cases occurred at Pretoria last year; there were ten cases in all, most of them coming from one company of a particular regiment. The clinical signs were similar to those noted in the former cases, the same eruption being present, but the course of the fever was shorter, lasting only for one to two weeks. In several cases in which blood cultures were made the results were negative, and the sera of the patients did not agglutinate *B. typhosus* or *B. paratyphosus, B.* From the fæces of one case a bacillus closely resembling *B. typhosus* was isolated. This bacillus differed from the stock laboratory *B. typhosus* in forming acid less rapidly in glucose broth, and in forming in litmus milk slight acidity succeeded by alkalinity. The serum of the patient from whose stools it was isolated agglutinated the bacillus, as did also the sera of the other associated cases. Unfortunately after a few subcultures this bacillus became very rapidly agglutinable by any serum, although when first isolated it did not agglutinate with control sera.

The third group of cases, some twenty in number, occurred at Roberts' Heights, in the early part of this year. Clinically they were similar to those already described, the same characteristic eruption being present. In this instance the cases were widely distributed over the garrison, including men and women, officers and their families. From two cases Major Statham isolated from the blood a variety of *B. coli* varying only from a typical *B. coli* in that the clotting of milk only took place very slowly. The sera of all of the cases in the group agglutinated this bacillus, which was unaffected by the sera of control cases. The agglutinating power of the sera rapidly diminished and disappeared in convalescence.

At the time these cases appeared, *B. coli* was found present in one c.c. of the water supply, and culturally was identical with *B coli* isolated from the patients, but the sera of the patients had no agglutinating power on the bacillus isolated from the water supply.

Lieut.-Colonel Maher, R.A.M.C., informs me that at Potchefstroom a precisely similar fever with characteristic eruption is common. Agglutination tests with *B. typhosus* and *B. paratyphosus, B.,* yield negative results. He says similar cases occur in the town of Potchefstroom, where they are usually diagnosed "influenza." In several cases he ascertained that patients had been bitten by ticks two or three days before the onset of the symptoms, and he suggests that the tick may have conveyed the infection. I may say I have stained blood-films from several cases of this fever but have never found blood parasites present.

I have seen some thirty cases of this type of fever, and have no doubt that it is a distinct entity and can be differentiated clinically from enteric fever or the recognised form of paratyphoid fever Its leading characters may be shortly summarised as follows:—

(1) The onset is sudden, and is accompanied by particularly severe headache, pains in the back and limbs, dark flush on cheeks. injected conjunctivæ, and suffused eyes. Sometimes the patient has been out of sorts for a few days before the onset of distinct symptoms, but usually the disease sets in quite suddenly.

(2) A characteristic dark-red maculo-papular rash, not unlike the rash of German measles, appears all over the trunk and limbs I have found this rash present on the soles and palms in all cases where I had an opportunity of looking for it. Interspersed with the characteristic rash are sometimes found lighter red spots like rose-spots. The rash leaves brown stains when it fades. It appears earlier than the rash of typhoid fever, sometimes coming out on the second or third day of illness.

(3) The tongue is coated, but not usually dry or brown. Constipation is usual, the motions have a dark-brown colour. The spleen is generally enlarged. There may be some abdominal fulness, but not marked distension.

(4) The fever lasts from ten to fourteen days as a rule. It usually comes down by rapid lysis, but may do so by crisis. Convalescence is rapid, and there are no relapses.

(5) Though the patient often appears to be extremely ill during the first week of the disease, yet I do not know of it ever proving fatal.

As regards diagnosis, these cases have been regarded as influenza, but beyond the severe headache and pains in the limbs there are no grounds for this diagnosis. They are not contagious as influenza is, nor are they accompanied by the respiratory complications or nervous and other sequelæ of that disease. The rash is striking and distinctive.

The sudden onset and rash recall dengue fever, which I have seen in Bombay, but dengue fever is intensely contagious, has distinct joint complications, and has a distinctive course, showing two separate fever periods, each with its characteristic rash.

The recently described "pappatasi" fever is a short fever, lasting only two or three days, is not accompanied with a rash, and, as I know from personal experience, is characterised by very severe pain along the course of nerve trunks.

This fever resembles typhus in its onset and duration. The rash is somewhat similar but macular. It is not contagious like typhus, is not dangerous to life, and we do not meet with the mental symptoms we find in typhus; though in the first few days the patient may be dull, this soon passes off.

Since writing the first part of this paper, I have read an article by Dr. Nathan E. Brill, in the *American Journal of Medical Science*, for April, 1910, describing "An Acute Infectious Disease of Unknown Origin," which appears to be identical with the fever I have just brought to your notice. Dr. Brill's paper is based on the study of 221 cases observed in the Mount Sinai Hospital, New York, during the past fourteen years. The incubation period is short, usually four or five days. The attack begins with intense headache, apathy and prostration, aching all over the body, chills, and sometimes vomiting. The eyes are dull and suffused, and the conjunctivæ congested. About the sixth day an erythematous, maculo-papular eruption makes its appearance on the back and abdomen, spreading to the limbs, and sometimes involving the palms and soles. The spleen is frequently enlarged; the tongue is coated with white fur, and the bowels constipated. The fever lasts a fortnight or less, and comes down by lysis or crisis. The disease appears in the summer season, and is not contagious.

The serum of the patient does not agglutinate with *B. typhosus* or with *B. paratyphosus, B.*, and frequently repeated attempts to isolate bacilli from the blood have proved fruitless.

Dr. Brill at first regarded these cases as belonging to the paratyphoid group but now considers they form a distinct class by themselves, their clinical features distinctly differentiating them, and the failure to isolate bacilli from the blood militating against their inclusion in the coli-typhoid group.

Dr. Brill's paper merits careful study, as I am certain the disease he so minutely describes is common in South Africa. Although in certain cases organisms of the coli-typhoid group have been isolated from the blood, I am not at all sure that we can therefore set down this fever as a form of paratyphoid. Its clinical course is so distinctive that one would expect to find it associated with one distinctive organism, and not now with one member of the group, and now with another. We must remember that in the cases of swine fever and fowl cholera the presence of certain organisms in the blood appears to be rather a concomitant than the cause of the disease, though the question as to whether the bacillus found in the blood or an invisible virus is the exciting cause is not yet definitely settled.

III.—SPIROCHÆTA PALLIDA AND SYPHILIS.

By W. E. de Korté, M.B., M.R.C.S. (Eng.), L.R.C.P. (Lond.)

(Not supplied for publication.)

IV.—ENVIRONMENT AND DISEASE.

By W. Watkins-Pitchford, M.D., F.R.C.S. (Eng.), D.P.H.

This is, I believe, the first occasion upon which Pathology has formed the subject of an undivided section of the South African Medical Congress. This new departure is fully justified by the ever-increasing recognition of the fact that our subject is of the first importance in successfully dealing with disease. Methods of empiricism in medical practice are being rapidly replaced by rational procedures founded upon the results of experiment, and traditional explanations of the phenomena of disease are being abandoned in favour of those which are deduced from systematic investigation. The universal adoption of scientific methods of observation and experiment has, in our generation, let loose a flood of new discoveries. Every sick man presents to his physician a complex of phenomena, and the more the physician knows of modern research the more elaborate does he find that complex. Necessary results of this, one might almost say tumultuous, evolution of medical knowledge have been the rise of the specialist and the birth of new sciences. Some idea of the rapidity and extent of this recent elaboration of our art may be gathered from the fact that in the year 1894 the Annual Meeting of the British Medical Association was divided into ten sections, whilst in the meeting of the present year there were twenty-one sections. This irresistible and steadily growing tendency for medical knowledge to break up into separate departments may be to the benefit of pure science, but it must very materially restrict the scope of the general practitioner. The "complete physician" has, in the past, occasionally been found; in the near future he will be an impossiblity.

It is customary with our colleagues in Western Europe to take advantage of such occasions as this to publish the results of their investigations in pathology. In this respect we in South Africa are at a great disadvantage, for systematic pathological enquiries are but very rarely undertaken, and, as individuals, we have few or no results to exhibit. We are but beginning to find out where our main lines of research must be carried, and are still without funds and facilities for the prosecution of the work. Research into the causes, prevention, and treatment of the diseases which affect the people has hitherto received no support from their elected representatives, whilst research into the diseases of their domesticated animals has been lavishly subsidised. The explanation of this anomaly is on the surface. We are a young nation, we have but recently opened a shop in the market-place of the world, and with all the recklessness of youth we concern ourselves more with the prospective sale of our goods than with our own lives. The degree of our unwillingness and want of preparation for the work of human pathological research in South Africa is a measure of the youth and inexperience of our nation.

To give a concise and at the same time accurate definition of pathology is difficult. We may attempt it by saying that it is that branch of medical knowledge which deals with the causes and characters of those dangers in the body which constitute disease. Like all other branches of modern knowledge, it has thrown out numerous shoots, some of which—such as those of morbid anatomy and histopathology—seem to have almost reached maturity, whilst others—such as pathological bacteriology and the "special pathology" of certain diseases—are growing so rapidly that they possess literatures all of their own.

It is not my intention to ask you to accompany me in a survey of the present domain of pathology; those of you to whom such a survey would be refreshing have doubtless made it for themselves with the aid of recent text-books. I shall confine my remarks mainly to the ætiological side of pathology, and shall attempt to suggest that this side of our subjest possesses, as it were, a topical aspect. The majority of us doubtless learnt our first lessons in pathology in the schools of the United Kingdom and Western Europe, and the materials used to illustrate our lessons were taken from the diseases of that part of the world. The questions which I propose to present for your consideration are: whether there are any disease-changes. always excluding those produced by parasitic organisms, which are causally attributable to those factors in our South African environment which are natural; whether the distinctive changes of any of the classical diseases are modified by our environment; and whether we possess any special opportunities for the prosecution of any particular branch of pathological enquiry.

Among the most striking features of our natural environment is the relatively low barometric pressure which prevails over a large part of the country. That is the result of elevation above sea-level, and we note that the majority of the important towns of South Africa, excluding the coastal towns, are situated at a relatively high altitude Johannesburg, Krugersdorp, Charlestown, Bethlehem, Witbank. Harrismith, Modderpoort, Greylingstad, Heidelburg, and Standerton are more than 5,000 feet above the sea-level; Middelburg, Bloemfontein, Kroonstad, Pretoria, Bulawayo, Potchefstroom, Klerksdorp. Pietersburg, Mafeking, and Kimberley are at an altitude of between 4,000 to 5,000 feet. Eight of the twenty towns we have now mentioned are situated above the level of Davos Platz, sixteen are above the highest point of Ben Nevis, and all are higher than the top of Snowdon.

A very brief consideration of the changes in the body which result from residence at high altitudes brings us face to face with the necessity of discriminating between physiological and pathological phenomena. Viewed from a purely philosophic standpoint, such discrimination is almost impossible; it will suffice for the present, however, if we agree to admit that a physiological change has a pathological significance when it tends to produce ill-health or aggravate disease.

The bodily changes produced by high altitudes have been investigated by numerous workers, and we will proceed to mention those of their principal conclusions which have been verified. At a sufficiently high altitude (1) the respiratory movements are more ample and frequent; (2) the pulse-rate is increased, and disproportionately so upon exertion; (3) the temperature is slightly raised; (4) the body uses more oxygen and the respiratory quotient is lowered; (5) the metabolism of fats and carbohydrates is increased and that of proteids diminished; (6) the increase of body-weight in the young is relatively more rapid; (7) the blood is capable of absorbing a larger amount of oxygen; (8) the erythrocytes are increased in number to a degree which is roughly proportional to the altitude; (9) the viscosity of the blood is increased; (10) the arterial tension is increased (this observation does not appear to have been widely confirmed: the immediate effect of reducing the barometric pressure is to produce a fall in blood-pressure); (11) the hæmoglobin is increased in quantity; (12) the red marrow of the bones undergoes hypertrophy.

The various observers are generally agreed that these several changes do not become easily appreciated until an altitude of 5,000 feet is reached; some, however, have recorded them at much lower altitudes. All are agreed that the changes are more pronounced and uniform in the young than in adults. There is practical unanimity in attributing the changes to the diminished tension of the atmospheric oxygen, and in considering most of them to constitute an effort of the organism to compensate for this deficiency in oxygen.

The air of a locality at the altitude of the Rand contains, of course, a less amount of oxygen than that of a district at sea-level. A litre of pure, dry air, at O°C., if examined in Durban when the barometer is standing at 760 mm. will be found to contain 297 milligrammes of oxygen. The same bulk of air, at the same temperature, in Johannesburg—where the average barometric reading may be taken to be 618 mm.—will be found to contain only 241 milligrammes of oxygen. We see, therefore, that there is roughly about one-fifth less oxygen in the air of Johannesburg than in that of a place at sea-level.

The most constant of the bodily changes produced by a diminution in the supply of oxygen, whether this be due to its reduced tension at high altitudes or to any other cause, is an increase in the number of the erythrocytes. To this reactive increase in the number of the red corpuscles the term "polycythæmia rubra" or, better, "erythrocytosis" (Hirschfield) has been applied. The degree of erythrocytosis consequent upon the altitude of the place of observation is illustrated by the following series of observations:—

Place.	Altitude (Feet).	Erythrocytes (per c.mm.).	Observer.
London	(?) 50	5,000,000	Gowers.
Tübingen	1,030	5,322,000	Reinert
Reiboldsgrün	2,278	5,900,000	Koeppe
Arosa	5,905	7,000,000	Egger.
Cordilleras	14,409	8,000,000	Viault.
Balloon ascents	15,000	8,800,000	Gaule.

The average number of erythrocytes of ten healthy adult males whom I examined at a place (Cleveland) near Johannesburg, with an altitude of 5,480 feet, was 6,864,000 per c.mm. Making use of the same instruments in Durban a few days later, the average number of erythrocytes in the blood of ten similar men was found to be 5,728,000 per c.mm. The existence of a definite erythrocytosis at the altitude of Johannesburg was therefore confirmed. We see, moreover, that, in a general way, the degree of erythrocytosis in this instance is roughly proportionate to the diminution of the atmospheric oxygen, there being an increase in the number of the erythrocytes by about one-sixth to compensate for the one-fifth deficiency in the amount of oxygen in the air.

This erythrocytosis of altitude appears to be directly brought about as a result of the increase in the bulk and activity of the hæmapoietic red marrow which inhabits the cancellous spaces of the bones. The change can be detected in the blood-count within twenty-four hours of a person's arrival in the higher country, and disappears, together with the other changes, in a few days after his return to a lower level. Now this erythrocytosis is not only the most constant, but is also probably the most important, of the results of diminished barometric pressure, for it entails other changes which may be of serious moment both in health and disease. Any increase in the total bulk of the erythrocytes—the volume of the blood remaining the same—must be at the expense of the plasma, and must therefore tend to make the blood less liquid and in consequence more viscous. Dr

Walter Frei has pointed out that the internal friction of the blood is chiefly dependent upon the number of the corpuscles suspended in it. The five million erythrocytes which are normally suspended in a cubic millimetre of blood at sea-level occupy just about one-half of the volume of the blood, and if, whilst they still retain their usual individual size, they are increased in number to about twelve million, the blood is reduced to the consistency of paste or wet sand. I remind you of these observations in order to emphasise the fact that the permanent addition of a million erythrocytes above the number normally present in a cubic millimeter must produce a definite change in the physical conditions of the circulation. Any increase in the viscosity of the blood necessarily impedes its flow through the vessels, especially those of small size, and this increased peripheral resistance brings about a rise of blood-pressure, and throws more work upon the heart. It is more particularly in the heart, therefore, of the person who lives under the reduced pressure of high altitude that we may expect to find physiological changes with a pathological significance.

In a paper read before the South African Medical Congress in 1907 Dr. E. P. Baumann called attention to the ease with which schoolboys in Johannesburg become exhausted by such physical exertion as cross-country racing. Dr. Baumann examined 310 schoolboys—selected without reference to athletic proclivities—and discovered that in 59 per cent. of them the heart was hypertrophied, in 6 per cent. there were evidences of this hypertrophy having given place to definite dilatation, and that in only 34 per cent. could the organ be considered as presenting normal signs. Dr. Baumann attributed these changes in the heart to the increase in the rate of respiration necessitated by the altitude of Johannesburg, with a consequent increase in the frequency and force of the heart-beats. It seems probable, however, that another and an important cause for this effect upon the heart is to be found in the additional impediment to the capillary circulations, both systematic and pulmonary, which the increased viscosity of the blood produces. As already stated, the increased viscosity is the direct result of the erythrocytosis.

It has long been recognised that people suffering from cardiac incompetence or chronic renal disease are specially liable to severe distress at even moderately high altitudes, and this liability is doubtless in part explained by the increased viscosity of the blood.

We may now ask ourselves the question whether high altitude unfavourably affects the course of lobar pneumonia. The toxæmia of this disease, and the characteristic changes in the affected lung-tissue, give rise, as we all know, to considerable embarrassment of the circulation. The fatal termination is usually attributable to right-heart failure, and it seems probable that enquiry might show the liability to fatal circulatory failure to be more pronounced when the patient is living under considerably reduced barometric pressure. Whether such an increased liability has been actually observed in Johannesburg practice I do not know, but in consideration of what we have already stated as to the influence of altitude upon the blood, the heart, and the circulation of the healthy individual, it is probable that it exists. In a recent article on "Mountain Sickness" Dr. Leonard Hill states that in Mexico City—the altitude of which is 7,450 feet—all visitors, except the young and perfectly fit, feel a certain amount of distress for two or three weeks after arrival. He also states that pneumonia is reported to be very common, and that the local medical men consider that the best chance of recovery is given when the patient is conveyed down, by train, to a station some 3,000 feet lower. If benefit results from such a procedure, it could doubtless be also achieved, and with less disturbance, by transferring the patient to a specially constructed room in which the pressure of the air could be artificially raised to, and maintained at, the requisite tension. Paul Bert many years ago showed that animals could be submitted to an atmospheric pressure seven or eight times that of the normal without any injury or impairment of functions. The observation has since been verified by physiologists who have used themselves as subjects. The changes in the body which arise during prolonged increase of atmospheric pressure have not been so exhaustively studied as those due to its diminution. It is probable that they are, in a general way, the converse of those changes: we find, for instance, that the pulse and respiration rates are slowed. The body-changes which constitute caisson disease are not produced during exposure to compressed air, but are almost always consequent upon too rapid a return to the normal environment; they are the immediate results of decompression, not of compression. It has been repeatedly proved that decompression symptoms do not arise when the increase of pressure to which the subject has been exposed has not exceeded that of an additional atmosphere; in such circumstances a rapid return to the normal pressure appears to be innocuous.

The relief afforded to the respiratory and circulatory processes by a slightly compressed atmosphere is beyond question, and has already been made use of therapeutically. At the Baths of Ems, near Coblentz, cases of chronic bronchitis, emphysema, and those which exhibit the sequelæ of pleurisy and pneumonia, receive what is termed the "passive pneumatic" treatment. The pressure of the air in the pneumatic ward at Ems is artificially raised by from 200 to 320 millimetres of mercury, *i.e.*, from five to eight nineteenths of an additional atmosphere. The treatment of whooping-cough in compressed air has also met with much success in France at the hands of MM. Rocaz and Delmas. Their séances last from half to one hour at an additional pressure of 300 mm. of mercury, *i.e.*, an increase of about seven-nineteenths of an atmosphere. It has not been found that severe bronchitis or cardiac dilatation contraindicate the treatment.

Apart from specific bacterial vaccines, the therapeutic agents which specially recommend themselves, on theoretical grounds, in the treatment of lobar pneumonia are bleeding and the administration of oxygen. One of the principal effects of bleeding is to reduce the viscosity of the blood; the volume of the blood which has been allowed to escape is speedily made up in the vessels by fluid from the tissues, whilst the corpuscles, to the presence of which the viscosity of the blood is principally due, are but gradually renewed. The administration of oxygen enables the blood to become oxygenated with less expenditure of respiratory effort, and thus indirectly relieves the heart of some of its unwonted task. Oxygen gas, however, if given too undiluted and under too great a pressure, may be injurious, and may actually originate the disease changes which we are proposing to modify. The theoretical advantages of both venesection and the administration of oxygen are, *a priori*, to be obtained by increasing the atmospheric pressure, and such an expedient is free from the disadvantages of lowering the vitality on the one hand and the risk of an overdose on the other. We have already remarked that increased respiratory movements and erythrocytosis—with its accompanying increase in the viscosity of the blood—are changes which are produced by high altitude, and as such rapidly disappear upon return to a low level, or, in other words, to an increased atmospheric pressure. I am not aware whether the suggestion to treat lobar pneumonia in compressed air has been yet carried into practice, or even made, but for the reasons which I have now laid before you it appears worthy of trial. At the mines of the Rand, where lobar pneumonia is certainly as prevalent and as fatal, if not more so, than elsewhere, compressed air is almost an industrial commodity, and the construction and maintenance of a small pneumatic ward, with air-lock, ventilated under pressure by a modification of the "plenum" system, would not be an impracticable matter. It is desirable that if such treatment be essayed, it should be commenced in the early stages of the disease—that is, before the fourth or fifth day—for in serious cases it is about this time that the blood-pressure begins to fall (Giglioli). Such an installation, even should it prove of no use in the treatment of pneumonia, would probably be of considerable benefit to cases of chronic bronchitis and heart disease. In any case, we suggest that the treatment should not, as at Ems and elsewhere, be confined to séances lasting an hour or so, but should be continuous.

I fear that in what I have just said I may have ventured too far into the province of therapeutics. Having pointed out, however, that the high altitude of many of our towns introduces a new element into many diseases, it is, I think, permissible to indicate the theoretically appropriate method of meeting this disadvantage; the primary employment of pathology should be, of course, in the service of the sick man.

It is certainly desirable that South African practitioners should be conversant with the ascertained effects of high altitude upon the human body, and we are all called upon to observe and investigate, whenever possible, its influence in disease.

I show you photographs of a comparatively simple apparatus by means of which animals may be conveniently kept at any desired barometric pressure. The air within the receiver is kept continuously changed by means of aspiration with a Geissler water-stream pump, whilst its pressure is reduced to, and maintained at, any desired point by the action of an adjustable mercurial barometer. This barometer is made for the most part of rubber pressure tubing. One arm of the barometer is in communication with the receiver, the other is flexible, and is attached to the valve. The valve is fixed in a frame which is capable of being moved up or down, and of being fixed at any height on a graduated scale. The surface of the mercury in this movable arm of the barometer is in contact with a stout elastic membrane which operates the valve connecting the receiver with the outer air.

If one or two individuals from a litter of young rabbits be kept in the receiver of this apparatus under reduced pressure, the following bodily changes can be readily observed:—increase of the red marrow of the long bones, erythrocytosis, increased viscosity of the blood, and cardiac hypertrophy. Animals from the same litter but kept in ordinary hutches should, of course, be used for control observations. The Pietermaritzburg Laboratory stands at an altitude of 2,400 feet, and allowance has to be made for this fact in examining the control animals. Here are the femora of a rabbit which lived for seventy-six days at a reduced pressure equivalent to an altitude of 10,000 feet, and here is the femur of a control animal of the same litter. In an experimental animal, after eight days' exposure to this degree of reduced pressure the erythrocytes numbered 6,648,000, after twenty-five days they numbered 8,080,000, and after fifty-three days, 9,123,000. In control animals the erythrocyte count was found to be about 5,000,000. The viscosity of the blood, when the erythrocyte count was 8,400,000, was 6.2, whilst in the control animal it was 3.9. To estimate the degree of cardiac hypertrophy by the absolute weight of the heart might be misleading owing to the variations in the size of the whole body in different animals of the same litter. I have therefore ascertained the ratio of the weight of the heart to that of the two kidneys, and found that whilst in the control animals this was .296 : 1, in the experimental animal, after seventy-six days at 10,000 feet, it had risen to .451 : 1.

Another prominent peculiarity of our environment is the relatively great amount and intensity of sunlight. The existence of this peculiarity is a commonplace assumption the validity of which is confirmed by such considerations as the following.

The City of London enjoys, approximately, 26 per cent. of the total possible amount of sunshine throughout the year. We find, on the other hand, that in Johannesburg the sun shines for, approximately, 72 per cent. of the total possible period (Innes). In places at a high altitude, and in those with a dry climate, the intensity of the sun's radiant power is usually much increased, a fact which is largely due to the relative deficiency of the air of such places in watery vapour. The black bulb thermometer (*in vacuo*) is an instrument which gives us a measure, in a rough and ready manner, of the actinic power of the sun's rays. In England the reading of this instrument rarely exceeds 150° F.; the highest reading in Durban during the year 1905 was 154° F. (Nevill), whilst in the same year in Johannesburg the highest reading was 164° (Innes). At a station in the north of Cashmere, with an altitude of 11,000 feet, the readings are reported to be not uncommonly above 212° F.

In 1893 Finsen drew attention to the fact that the most numerous and the deepest scars which are left by an attack of small-pox are usually found upon the face and the backs of the hands. Upon this observation he based his hypothesis that actinic light increased the cutaneous inflammation in this disease. The publication of these views led, as we all know, to the revival of the "red light" treatment of small-pox. Pursuing his investigations on experimental lines, but in the opposite direction, Finsen introduced, in 1897, his conspicuously successful "actinic light" treatment for lupus vulgaris. Thus the work of one man has demonstrated the fact that actinic light may aggravate one disease and alleviate another. We may make a topical application of the principles of Finsen's observations by enquiring whether small-pox, in the unvaccinated, is not a more severe disease, and lupus vulgaris a less severe disease, with us than in the North-West of Europe.

Finsen's deduction concerning the existence of actinic influences in variola may, it seems to me, be extended with considerable justification to include certain other "systemic" diseases accompanied by cutaneous manifestations. In scarlet fever, measles, rubeola, and epidemic cerebro-spinal meningitis we find that the specific rash has a marked tendency to appear first on the face, neck, and upper part of the chest—that is to say, upon the parts of his body which the patient exposes most continuously to the light. The marked tendency which the lesions of the "tubercular" form of leprosy have to affect especially the face and hands, and the fact that the herpes which is so often associated with lobar pneumonia, infectious catarrh, and bronchitis, is almost always a facial herpes, are also suggestive of the same influence.

More than twenty years ago Virchow recorded the fact that cattle which had eaten buckwheat (*Fagopyrum*) may develop a cutaneous eruption. It was noted, however, that this eruption only appeared in white or lightly pigmented animals, and in these only when they were exposed to the daylight; when the animals were kept in dark sheds the eruption did not appear. This observation is one which seems to merit the most careful attention of those who would investigate pellagra; it is, moreover, one which we ourselves may repeat, as buckwheat is grown in considerable quantities in certain parts of South Africa.

Pellagra is probably a disease in which the influence of actinic light is more clearly shown than in any other. The disorder is not uncommon in Egypt, and not unknown in South Africa. Its first undoubted manifestation is an erythema of the face and neck, the backs of the hands, and any other region of the body habitually exposed to light. In those who wear no boots the dorsum of the foot is affected, whilst in those who, like the Egyptian fellaheen, work in the open in an almost nude condition, the skin of the greater part of the body develops the characteristic changes. To make the conclusion absolute, as it were, we note that Scheiber has observed that when a person who wears rings falls a victim to pellagra, although the fingers may be severely affected, yet the skin immediately beneath the ring is of normal appearance.

Actinic illumination is certainly not the specific cause of the exanthemata, and probably not the sole cause of pellagra, just as darkness is not the specific cause of lupus; both light and darkness are, however, agencies which modify not only these diseases but also, in all probability, many others.

What influences the agency of excessive natural illumination may possess in various diseases should be especially observable in our country. In this respect, however, the absence of reliable statistics of past experience in South African hospitals, or perhaps more correctly, the absence of reliable compilations of such statistics, is a serious impediment to pathological enquiry. We cannot, for instance, give any statistical support for, or refutation of, the statement that duodenal ulcer is relatively common with us. If this or other disease of obscure or questionable ætiology could be definitely proved to be more common or more severe, the fact would go far to justify an enquiry into the possible influence of actinic light in its development.

We may now ask whether there are any diseases which are admittedly, not merely influenced, but caused by actinic light *per se?* In approaching this question, we are again confronted by the difficulty of distinguishing clearly between the physiological and the pathological, and must admit that the boundary line is not rigidly fixed. Whether, for example, we deem the freckles on the face and hands of the xanthochroic individual a phenomenon of health or a very mild form of disease is merely a matter for individual choice. The formation of pigment in the skin upon exposure to strong light is evidently a purely physiological reflex, a protection against a familiar enemy; on the other hand, the severe dermatitis which may precede the appearance of the pigmentation in blondes is a definite pathological lesion.

The following conditions are generally considered to be pathological, and are admitted, by universal consent, to be mainly the result of actinic radiation—" sailor's skin " of Unna, xeroderma pigmentosum of Kaposi, X-ray dermatitis, pinta cœrulea, ephelis *ab sole* and *ab igne,* erythema and eczema solare, snow, ice, and arc-light blindness. All these diseases appear to affect blondes more readily than brunettes or coloured people. With the exception of the retinal affections, they are all characterised by the development of varying amounts of obvious pigmentation, and at least the first three of them have a definite tendency to terminate in cutaneous carcinoma.

In Unna's " sailor's skin " the ears, cheeks, temples, neck, and backs of hands are first marked with a diffuse erythema; patches of pigmentation then appear, the skin becomes rough, thickened, and leathery, and sooner or later carcinomatous outgrowths commence. A similar condition of the skin of the exposed parts of the body may be found, not infrequently, I think, among those South African farmers who are of fair or reddish complexion. I have not seen it in a person of dark complexion or a coloured person, although, at the same time, it appears to be the rule for albino Kafirs to show these changes to a greater or less extent.

Xeroderma pigmentosum is another of the recognised light-produced diseases for which South African practitioners should be on the lookout. Unna considers it as essentially identical with his " sailor's skin," but occurring in the young instead of in the middle-aged or elderly. The disease commonly commences about the second year of life with an erythema, followed by discrete pigmentation (freckling) of the exposed parts of the body; warts and small angiomata are developed, and finally multiple carcinomatous growths destroy the child before it has reached maturity. I show you a coloured illustration of this interesting condition; it is taken, as you will see, from Hyde and Montgomery's work on the skin. In his essay " On the Influence of Light in the Production of Cancer of the Skin," a copy of which the author kindly sent me in July of the present year, Dr. Hyde adverts to both Kaposi's xeroderma and Unna's " sailor's skin," and from other additional evidences arrives at conclusions very similar to those which, drawn in the main from different premises, I ventured to lay before the South African Medical Congress last year.

Mr. G. Lenthal Cheatle has called attention to the appearances which may very commonly be observed in the skin of the hands and face of old people, especially in the country districts in England. The skin is thin and shiny; it is irregularly pigmented and apparently scarred, although there is no history of traumatism to account for the apparent scars. The position in which pigmentation is most commonly and extensively developed is on the back of the hand over the second metacarpal bone. The condition very much resembles the bronzing of the skin resulting from prolonged exposure to X rays, and the two affections are also similar in that they are prone to terminate in cancer. These apparently senile changes were also described by Dr. Hyde, of Chicago in 1906. Mr. Cheatle has given the name of "biotripsis" to this condition, and accredits it with a neurotrophic origin. It is apparent that the changes of "biotripsis" have a considerable analogy with the lesions of "sailor's skin," the principal difference being in the absence of thickening and roughening. In respect of this difference, however, it is to be observed that a keratosis of the hands and face of elderly people is recognised in the United States of America under the name of "senile keratosis."

In Kaposi's disease "sailor's skin," X-ray dermatitis, and "biotripsis," the association of actinic stimulation, patchy pigmentation, and new-growth is exemplified; and the more one considers the circumstances of this association the more one feels that we are here within the penetralia of the mystery of cancer. The temptation to discuss these matters is great, but time and the occasion forbid more than one or two brief remarks.

It seems evident that the pigmentation in these conditions represents the physiological, reflex process tending to provide protection. Although we are so familiar with the occurrence of this protective reflex that we unhesitatingly consider it as "normal," yet we must not forget that it is, biologically, a somewhat singular phenomenon, inasmuch as it is only in one variety of one animal that it is at all conspicuous. The white man is the only daylight, terrestrial mammal with a thin, unprotected, translucent skin, and he is also the only animal, as far as I know, whose skin will turn brown after a few hours' exposure to sunlight.

It seems probable that the presence of discrete deposits of pigment may, apart from actinic stimulation, be instrumental in developing cancer, and in view of this fact we suggest that the "cell-riot" of cutaneous cancer may be produced in one of three ways. It may be due (1) to actinic stimulation, despite abortive attempts at the formation of a protective pigmentation; (2) to the presence, *per se,* of discontinuous deposits of pigment—whether these have been formed under actinic influence or not; and (3) to a combination of these two factors.

An illustration of the unequivocal sequence of carcinoma upon actinic stimulation seems to be afforded by those cases of lupus vulgaris which, whilst undergoing treatment by the Finsen light, have developed carcinoma at the seat of treatment. The association of new-growth with a previous deposit of pigment, which has not been obviously developed in response to irradiation, is exemplified by such conditions as acanthosis nigricans and nævo-carcinoma. The former of these cutaneous affections is especially noteworthy, inasmuch as the life of the patient is usually terminated by carcinoma of the stomach or uterus, the viseral cancer being apparently unconnected, by metastasis, with the cutaneous growths. The cutaneous cancer known as nævo-carcinoma arises from a pigmented mole, and is held by Unna to be a true carcinoma, and not a melanotic sarcoma, as was formerly taught. It is possible that in pitchworker's disease the plugs of extraneous pigment which fill the sebaceous ducts, and the carcinoma which is developed around the duct, also exemplify the same association. Another example of the sequence of neoplasm upon non-actinic pigmentation is observable, occasionally, when arsenic has been taken in large doses or over long periods. In such circumstances irregularly distributed pigmentation appears in various regions of the body surface, including the palms and soles, keratosis supervenes, and, in a few cases, is followed by the development of definitely malignant papillomata.

In view of the causal association of arsenic with both pigmentation and new-growth *in the skin,* I think the following case is worthy of mention:—

In May of last year an Indian storekeeper in Maritzburg came into the possession of a quantity of white arsenic. Not knowing what it was, but appreciating its whiteness and weight, he mingled it with the contents of his flour-bin, and sold the mixture to confiding natives. Of the many victims of this commercial venture, one, a healthy young Zulu, aged twenty, almost lost his life from acute arsenical poisoning In a few days he appeared to have recovered, only to die, however, some four weeks later, with a rapidly developed enlargement of the liver. A *post mortem* examination showed that nearly the whole liver had become converted into a mass of carcinomatous new-growth, and that there was no primary growth on the surface of the body from which the neoplasm in the liver could have been derived from metastasis.

We will now briefly suggest, as a subject for future enquiry and observation, that the mildness, or rarity, of certain diseases in South Africa may be due to the beneficial effects of bright light upon the body. If this could be definitely proved, the observation would have an important application, both for treatment and prevention, in the gloomy countries in which such diseases are more severe or more frequently met with.

At the annual meeting of the British Medical Association which was held at Toronto in 1906, Dr. Hyde of Chicago proposed that psoriasis was " an expression of the resentment of the skin " against partial or total exclusion of light. I must leave it to those who have not done so to read his most interesting paper in the back numbers of the *Journal.* In the discussion which followed the reading of the paper Dr. James Ross of Halifax, N.S., referred to several cases of the disease in coal miners, and it was also suggested that the Negro races enjoy a relative immunity from it. We are justified, I think, in supposing that the disease is relatively infrequent in South Africa as compared with the Old Countries, but especially Scotland. Whether those who spend much time underground, or within doors, are more liable, and whether the South African Negro is less liable, than others are questions which we should also be inclined to answer in the affirmative.

Fresh cows' milk is a commodity which, with us, is rarer and more expensive than in the cities of England. Despite this fact, and notwithstanding the poorness of our potable waters in lime, it seems as though rickets were relatively very infrequent in the white children of South Africa. A verification of this apparent divergence, and of the causal inference which I have suggested in explanation, would secure an advance in our knowledge of the ætiology and treatment of the disease.

Other non-infective diseases, such as acute gout, osteo-arthritis, and eczema, and some infective diseases, such as scarlet and rheumatic fevers, will naturally occur to most of us as being relatively mild or rare in South Africa; all these call for a similar critical comparison. As, however, the principle of such enquiries has now been sufficiently illustrated, it would be waste of time to detail further surmises. They can only be accomplished by statistical investigation, for a mere opinion unsupported by mathematical evidence is of but little value. We have to admit, with regret, that, at the present time, materials for such investigation are not available in the records of our private and hospital practices. It is desirable that an organised enquiry should be undertaken, on the other hand, into the diseases which excessive natural illumination may produce or aggravate, and, on the other hand, into the incidence of those diseases which are severe in North-Western Europe but rare in our country, in order to ascertain what part light may play in mitigating or preventing them. The influences of both excessive and deficient illumination in the processes of disease are but little understood or heeded, they nevertheless constitute a field of research easily accessible and of special interest to us. Let us not suppose that such researches would have but an academic interest. Those who have read Colonel Woodruff's fascinating book, "The Effects of Tropical Light upon White Men," doubtless remember his main argument. It is to the effect that whilst the fair-haired, blue-eyed man from North-West Europe may, by his superior intelligence and energy, overcome and rule the inhabitants of tropical and subtropical lands, yet his stock speedily dies out when he attempts to colonise these countries. The proposition is a rational one, and urgently invites statistical confirmation in South Africa. In pursuance of Colonel Woodruff's general proposition, we have to ascertain whether a third or fourth generation of blondes is not unusually rare in South Africa? If this proves to be so, then an investigation into the diseases which destroy the blonde stock would become one of prime political importance. Such an enquiry would naturally be largely coincident with the enquiry into the nature of the diseases which are caused, or adversely influenced, by actinic light.

In addition to the peculiarities of high altitude and strong light, there are other factors in our environment which, though perhaps of subsidiary importance, are yet deserving of attention. We will very briefly refer to three of them, *viz.*, dryness of air, dust-storms, and softness of water.

It has been suggested that clavus and tylosis will be found to be rare conditions in moist, warm climates. It is for us to ascertain whether the dry atmosphere of the more arid regions of our country is not an important accessory in the production of corns. This homely complaint has seemed to me to be prevalent with us at altitudes of 2,000 feet and over.

In a paper which I had the honour of reading before the Public Health Section of this Congress in 1908 it was proposed that the pathological changes which characterise spring catarrh, pinguicula, and pterygium, were initiated by the dust-storms of our dry seasons. In the presence of excessive illumination, but apart from such influences, these conditions seem relatively rare.

When we compare the waters of our streams with those of English rivers, we find that one of the most prominent differences lies in the amount of chalk in solution. Speaking quite generally, it appears that the water supplied to London by the New River Company contains about ten times as much chalk as an average sample of South African drinking water. Now, this deficiency of lime in the water which we drink and in which our food is cooked may be held to explain the relatively slight tendency to the formation of tartar upon the teeth.

and will also probably account for the rarity of calcareous degenerations in pathological formations. In one's own experience in Natal, true calcification of atheromatous arteries and valves, uterine myomata, and tuberculous lesions, has appeared to be unusually infrequent. In those parts of South Africa where this pathological phenomenon is noticeable I suggest that more than the usual amount of chalk is present in the water.

In conclusion, I think that we should appreciate the fact that, as South Africans, we possess material for the study of certain pathological problems of paramount importance. I refer to the problems of racial liability. We have in this country unusual opportunities for contrasting the incidence and characters of diseases in the white and uncivilised black races of mankind. To compare two phenomena, to ascertain a difference, and to discern the cause of the difference is a fundamental procedure in the winning of all knowledge. If, for example, we find it authoritatively suggested that gastric ulcer, or psoriasis, are relatively very rare in the Negro, and if we ascertain by systematic investigation that the proposition is mathematically corroborated, we shall have possessed ourselves of raw material of guaranteed value for further research. Should a continuance of the enquiry disclose the reasons for the difference in liability, we should have added very considerably to our knowledge of the causes of these diseases in the white man. There are some who seem to maintain that the white and black races are equally liable to all non-infective diseases, and that the existing misconceptions have arisen from insufficient observation of the Negro. Others are of the opinion that a real difference in liability will be found to exist, and that such diseases as cancer, psoriasis, insanity, epilepsy, diabetes, glaucoma, cholelithiasis osteo-arthritis, gastric and duodenal ulcer, appendicitis, dental caries, and many others could be submitted to a comparative investigation with fruitful results. *Post mortem* examinations in hot countries are usually very inconvenient, and are therefore rare, a fact which has largely contributed to our want of exact knowledge concerning disease in the uncivilised Negro. For the purpose in hand it is evidently of first importance that no opportunity should be lost of making, and recording the findings of *post mortem* examinations of the bodies of Natives. One source of fallacy which is often noticeable in the fragmentary observations which are published from time to time on racial liability is the failure to discriminate between the Negro of pure pedigree and the merely " coloured " person.

The significance of racial liability to disease seems so great that its investigation justifies some special and systematic undertaking. If a small native community, living in its hereditary environment, could be selected and submitted to methodical observation, both statistical and medical, for a few years, many disputed points would be decided and much valuable knowledge would be acquired.

Until the time when the State will lend its support to such a scheme, and will facilitate the organised investigation of the influences of our environment upon our diseases, it must rest with each one of us to contribute what few observations he can to lighten our present ignorance.

V.—A CASE OF EPILEPTIC CONVULSIONS WITH DEMENTIA CAUSED BY THE CYSTICERCUS CELLULOSÆ.

By R. Sinclair Black, M.A., M.D., D.P.H., Valkenberg.

The patient had been a healthy woman up to the birth of her third child, when she was 25 years old. She then began to take epileptic seizures, becoming gradually confused in mind; her mental state got so bad that in 1902, when she was 34 years old, she had to be certified as insane, and from that date had been resident in Valkenberg till her death in 1910. On admission her mental symptoms were severe. She was suicidal, aggressive, flying into ungovernable fits of rage, meddling with fire, and, generally, a source of danger to herself and others. Confusion was also a prominent symptom, she thought a pillow was her baby, and wanted milk for it; was incoherent. She was so excited on admission that no detailed examination could be made. She was classed as an ordinary epileptic. Her convulsions were general; no special or partial distribution of them was noticed, and she was put on treatment by bromide of potassium. Dementia was from the first a marked feature, and she could not converse or employ herself. Gradually the dementia deepened into stupor; she became paretic, and for many months before her death lay in bed with her limbs drawn up into grotesque positions. There were now frequent general tremors and marked shakings. From her admission the fits were severe and frequent, usually twelve to twenty during a month; at the last she at times fell into the *status epilepticus,* and at one of these periods died of exhaustion. There was no special indication that her case was not idiopathic epilepsy till the last, when the contortions of limbs set in. She was not noticed nor reported to be affected by tapeworm.

At the necropsy the following conditions were found: Nothing of special interest in the thoracic or abdominal organs. In a similar case which I was fortunate enough to light upon in a Robben Island Asylum patient, and which was published in the *Journal of Mental Science* for 1903, the bladder worms were found in large numbers in the muscles; but in the present instance none were so located. The skull was slightly asymmetrical, probably from internal pressure, the convolutions were flattened from pressure, and there was excess of cerebro-spinal fluid. The membranes were fairly healthy and not adherent except where cysts were situated; on the dura mater of the petrous portion of the right temporal bone was an adherent cysticercus cyst. Embedded in the meshes of the pia, and springing from it, and often partially embedding themselves in the cortex to which they were often firmly adherent, were numerous cysts of sizes varying from that of a pea to that of a large bean. The walls were generally thin, semi-transparent, and of a faint yellowish colour. Inside was a semi-gelatinous fluid, and in many the head of the scolex was clearly visible; in others the consistence was leathery, and in some the walls had collapsed. The cysts were distributed generally along the course of the anterior and middle cerebral arteries, and at the base round the circle of Willis. They were very numerous between the anterior lobes of the cerebrum and in the Sylvian fissure of both hemispheres and round the crura cerebri. On the surface of the frontal and parietal lobes they were more sparingly distributed. Only a few were seen within the lateral ventricles. Roughly calculated, there were 150 to 200 cysts, in the majority of which the scolex was clearly distinguishable through the thin walls. That they were the cysts of the *Tænia solium* was evident both from their large number and from visible scolices in the interior of the cyst. Where did they come from? It appears that a person who has a tapeworm in the intestines cannot derive Cysticerci directly from its ova; these must first pass through the stomach, where their shells are removed by the action of the gastric juices. It must therefore have been that this unfortunate woman had swallowed large numbers in dirty drinking water. Why the free ova should have emerged from the blood-stream at the cerebral circulation only it is difficult to conjecture; in my former case they were found generally throughout the muscular system. But there are many difficult points about this occurrence, one being the lengthened period—seventeen years—in which the cysts appear to have resided in the brain, presuming that the early epileptic condition was caused by them.

As to auto-infection, of course it is possible for one or more of the ripe joints to be regurgitated into the stomach, where the embryos are set free by the digestion of their cases.

VI.—A CASE OF CONGENITAL ABSENCE OF THE RIGHT LOBE OF THE CEREBELLUM.

By R. Sinclair Black, M.A., M.D., D.P.H., Valkenburg.

T. L., an inmate of Valkenberg Asylum for four years, suffering from senile dementia with an alcoholic history, was found at the necropsy to have the right cerebellar lobe congenitally absent in nearly its whole extent. There was nothing in his appearance, development or gait to suggest such a condition. He had been an intelligent man, was by occupation a soldier and held rank as sergeant. He died at the age of 74, of cerebral hæmorrhage. The cerebrum was normal in development. The general state of the central nervous system was that usual in senile dementia. A vessel at the right internal capsule had ruptured, causing extensive and destructive hæmorrhage.

That the arrested development of the right cerebellar lobe was congenital was shown by the size of the right cerebellar fossa of the occipital lobe, which was extremely small; in fact, the right half of the occipital bone itself was atrophied. Beyond the changes mentioned there was no sign of disease in the cerebellum or its membranes. The left lobe was normal in all respects, but from the sulci longitudinales superior and inferior the right lobe, or what represented it, tapered rapidly away, the size of this portion not being larger than the tip of the forefinger. The essential structure of the lobe was shown by the diminutive fissured cortex, the medullary centre, and an almost imperceptible nucleus dentatus. Of the divisions of the lobe, the amygdala and the flocculus could perhaps be faintly identified. The middle peduncle on the right side was minute, and the pons on the same side was correspondingly flattened; the superior peduncle was hardly distinguishable, and from the shrinking of the restiform body the appearance of the medulla exteriorly and in horizontal sections was conspicuously altered.

Such a malformation as I have described is of extremely rare occurrence, and very few instances have been recorded. As I have said, there were no physiological abnormalities corresponding to the anatomical, the left half of the cerebellum acting perfectly for both sides of the body. What the cause of this abnormality might be it is, of course, impossible definitely to say, but the most likely condition would be partial obliteration of the cerebellar circulation on the right side in early fœtal life.

G.—SECTION OF SPECIAL SUBJECTS.

ON HOSPITAL ADMINISTRATION AND ORGANISATION IN SOUTH AFRICA.

By Sir Kendal Franks, C.B., M.D., F.R.C.S.I., Johannesburg.

Mr. President and Gentlemen,—

When you paid me the compliment of inviting me to open a discussion on "Hospital Administration and Organisation in South Africa," I little realised the magnitude or the complexity of the questions involved.

To deal with the subject comprehensively would require more time than this Congress could possibly allow, while merely to outline all the questions which arise in connection therewith would help us but little towards arriving at a solution of any one of them. I have, therefore, thought it best to attempt neither of these methods, but to confine myself to two of the most burning and perhaps the most controversial questions which the subject suggests. And here let me say that it is high time that the profession should tackle this subject, because hospital administration and organisation in this country is in a most chaotic state.

There are few questions which are more vital, or of greater importance to medical men in South Africa, than this question of our hospitals; and medical men themselves are to a great extent to blame for the present state of affairs. Owing to their education, their training and their experience they ought to have a very good idea of the broad lines upon which a hospital should be run, and the broad principles which should govern the administration. It cannot be expected that the average layman can have the necessary knowledge or experience, at least in a new country like this. In older countries, like England, there are a number of philanthropic gentlemen who have given up a large portion of their time to the study of hospital management, who have devoted a great portion of their lives to work of this kind, and whose experience and efforts have been, and are, of the greatest benefit to the community. This cannot be said of South Africa. There are, no doubt, many of a philanthropic turn of mind who, with the best intentions, have taken up hospital work, but who have had no experience and little knowledge to guide them. Had they taken medical men into their counsels, especially those who had had more experience of hospital work elsewhere than others, had they been content to avail themselves of experience and knowledge which they did not themselves possess, there would have been fewer mistakes, less bickering and unpleasantness, and less chaos in hospital administration than, unfortunately, exist to-day in South Africa. How it has come about that in the earlier days, when hospitals were first started in this country, medical men allowed them to be started on wrong lines; how it was that in the interests of good management—that is, in the interests of the public—they did not assist themselves more, but allowed all power and control to pass into the hands of well-meaning but inexperienced philanthropists, it is now difficult to say; but from what I have seen in this country, and I have seen a good deal, I would say that the chief fault lay in the want of cohesion among medical men themselves, in the want of a more defined *esprit de corps,* in a spirit of distrust and suspicion which permeated their ranks and which left them helpless, and their experience and advice negligible quantities. Hence it is that I say that the medical profession is itself largely to blame for the chaotic state of our hospitals to-day. The philanthropic layman has not been slow to note these things for himself, and to act accordingly. He has found the profession divided against itself. If he has sought advice, the advice has frequently been contradictory, and not always disinterested; and so, he has got into the way of depending upon his own judgment, and his own uninstructed and inexperienced intelligence, which have often landed him, and the institution he has undertaken to manage, into the most hopeless muddle. Professional dissensions have engendered in the mind of the layman a spirit of suspicion and antagonism, which we must henceforth lay ourselves out to dispel—for, believe me, we shall be able to do little to improve our hospital system until we bring about a more wholesome relationship between the profession and the laymen who have undertaken the management of our hospitals. There is, unfortunately, no exaggeration in what I have said as to the relationship existing among medical men. A short time ago I was talking over such matters with a barrister, and he lamented the manner in which medical men speak disparagingly of their colleagues. He said:—"If a layman were to speak to a barrister of another barrister in the terms in which I have heard one doctor speak of another, he would be at once shut up, in a way which would surprise him, and he would learn never to do the same again." It has been said, and I believe truly, that the members of the medical profession, as a whole, are the best read and the most highly educated gentlemen that are to be found in any profession. Their calling is acknowledged to be the highest and the noblest. Then why are members of our profession classed, it may be unconsciously, as inferior to members of other professions, such as the Bar, or the Army? I believe it is mainly, if not entirely, due to petty jealousies, to a small-mindedness that grudges to recognise merit in another, and that fears that every suggestion of appreciation of a colleague must necessarily detract from and injure oneself. Pardon me for this apparent digression, because I believe that to these disintegrating forces our powerlessness in the face of hospital mal-administration is almost entirely due.

Now, gentlemen, in approaching the subject of Hospital Administration we must recognise the fact that the conditions of a country like this render it impossible to follow closely the systems which hold in the older countries of Europe. Here we have a vast extent of territory, larger than France and Germany put together, but with a white population considerably below the average of an English County. To provide the funds necessary to establish and efficiently maintain hospitals in this country, it would be impossible to depend entirely, or even to a great extent, upon voluntary contributions. It would be most unfair, even if it were practicable, to put the burden upon the shoulders of the few who were charitably disposed, whilst those who would benefit most by hospitals would escape scot-free. The method which was adopted in the Transvaal previous to the war was, I think, the best, because it placed the burden upon the shoulders of those who could afford to pay, and as nearly as possible in proportion to their means. The Johannesburg Hospital owed its origin in the first instance to the necessities of a large mining area, and in the second place to the generosity of a few who realised the need. When approached for assistance, the Government gave the ground. Funds for erecting a small hospital, and then for enlarging it, were readily forthcoming, but it soon became apparent that a permanent source of income for the upkeep and maintenance of a rapidly growing hospital was absolutely necessary. The Government of the day could not see its way to granting a subsidy, and it was obvious that it was impossible to depend upon voluntary contributions in a community which was not only fluctuating in itself, but whose financial position was subject to the most sudden changes. Then it was that the leading men on the Rand devised a scheme which they laid before the Government, and which was adopted by them. The Government, for purposes of revenue, had imposed a tax of one shilling per month on every native who came to the Rand for employment. This tax was paid by the employer. It was proposed by the leaders of the Johannesburg community that the tax on the natives employed should be raised to 2s. a month, still paid by the employer, and that the revenue derived from this increase of the tax should be earmarked for the support and maintenance of the hospital. In this way the man who was in a position to employ most natives contributed more largely than he who could employ but one or two, whilst the poor escaped altogether. The income from this source was not very much the first year, but it kept on increasing year by year until it reached a sum of over £26,000. The Government then interposed and said, practically, that it was not good for the hospital to depend upon such a fluctuating source of income, and that therefore the Government would take over the tax, and would grant instead a subsidy of £26,000 a year to the hospital. As the fund continued to rise, and was believed to have reached over £100,000 a year, we felt somewhat aggrieved, because as the tax was levied on the community, at the request of the community, for a special purpose, we felt it should be devoted to that purpose. After the war, under Crown Colony government, we found some difficulty in getting the requisite funds for hospital maintenance, and when we appealed to our just claim upon the Native tax, we were told that this tax now formed part of the consolidated taxation of the country, and that we had no claim upon it. Since then, and under responsible government, the hospital has practically become a Government hospital, financed by the Government, supplemented by the receipts from paying patients and by charitable donations, the latter being almost a negligible quantity. I have referred to the Johannesburg Hospital because I know most about it, having been connected with it for a number of years, and because it illustrates what, in a country like this, I think would be the ideal method of supporting our hospitals. To finance all our hospitals through voluntary subscriptions is practically impossible; nor do I want to see our hospitals supported by Government, out of the general revenue of the country, as the Johannesburg Hospital is to-day. I think the nationalisation of our hospitals would be a calamity, and would lead to consequences disastrous to the interests of the sick poor, injurious to the hospitals, and cramping to the profession. The maintenance of our hospitals by Government out of the general revenue of the country would be a long step in the direction of their nationalisation. To my mind, a special hospital tax, which would fall not upon the whole community, but upon those who could afford to pay, like the Native tax, paid by the employers of native labour, would be the ideal way of raising funds for our hospitals. On the face of it there may not seem to be much difference between the support of our hospitals by Government, through a special tax, such as I propose, or out of general revenue, but in reality there is a vast difference. Our general hospitals are essentially charitable institutions, intended for the relief of the sick poor. In them the poor receive gratuitously all the comforts of a home, skilled nursing and the best medical and surgical treatment that the country can provide. The members of the staff, without fee or reward of any kind, give of their best to those who are admitted, because they are sick, and they are poor. When those who can afford to pay take advantage of these institutions as free patients, they defraud the poor, they cheat the tax-payer or subscriber who supplies the funds, and they wrong the medical man. This is what we mean when we talk of our hospitals being abused. It has reached such a pitch, especially in England, that it has become a very serious problem in almost every civilised country to devise means for checking it. In England some well-to-do people claim a right to free hospital treatment because they subscribe to the Hospital Saturday, or the Hospital Sunday Funds. The Socialist wants to have hospitals supported out of public funds, so that everyone who pays taxes may have the right to free hospital treatment, regardless of his financial position. It is not my intention to discuss this proposition, as to do so would raise the whole question of Socialism. The evils of such a system are so great that we should resist any attempt to introduce the thin edge of the wedge. Owing to this abuse, those " noble medical charities, scattered throughout England . . . . which are the wonder of foreigners and the just glory and pride of the British nation," have recently been described, especially in reference to the out-patient department, as " Schools of Pauperism." That is to say, that owing to the laxity of administration, or the failure to devise means for checking, or for controlling, the abuse of our hospitals, an ever-increasing number of our people, who formerly could hold up the head of independence, self-reliance and self-respect, are being educated into the ways of pauperism, and of dependence upon the charity of others for relief, which they frequently can, and ought to be able to, supply for themselves. This indiscriminating extension of free relief to those who ought not to require it is directly injurious to the poor; but I think its worst effects are seen in the deterioration of the moral fibre, of the self-respect and independence of a large number of the community. These people no longer think it necessary to make provision for medical treatment in case of illness or accident. They think it a hardship to be asked to pay in proportion to their means, and some will even resort to the most unworthy subterfuges to escape payment. They feel no shame in accepting charity, when it is at the expense of a hospital or of a medical man. The fact is that the medical profession itself, and those who manage the large charitable institutions, are very much to blame for this state of affairs—the profession in that, in the past, it has carried its charitable instincts too far, has been too ready to allow sentiment to override its judgment, very much in the way that many soft-headed philanthropists do an incalculable amount of harm by the indiscriminate distribution of funds; the managers of these hospitals, by a lack of system, or the employment of an inefficient system of regulating the admission of patients to free beds. In America a much better condition holds. There they discourage the indiscriminate admission to free beds, with the result that they are just as charitable as we are, they care for the sick poor just as much as we do, but they do not cultivate or encourage pauperism and they maintain the independence and self-respect of those who are not paupers. They do this in several ways. They limit the number of free beds. They have as ample hospital accommodation as we have, but there is a very remarkable difference between the number of free beds per thousand of the population in America to what is found in most European countries. Statistics show that the average number of free beds per 10,000 of the population in the seven largest towns in America is 15.5; whilst in London it is 20, in Newcastle 35, in Glasgow and Edinburgh 36, in Dublin 64, whilst if we go to the continent of Europe we find the proportion in Berlin is 38, Breslau 90, Stuttgart 114, Strasburg 145; in Paris it is 98, and in Rome 180. These figures show that hospital abuse exists in every country. It is recognised as an abuse in all, and all have for years been seeking for a remedy. The comparative success which America has achieved may partly be due to the greater love of independence in the individual, and a keener sense of what is due to his own self-respect; it may partly be due to the efforts of hospital authorities to instil a feeling of shame in those who try to take advantage of a charitable institution without paying for it, though able to do so; but it is mainly due, as Sir Henry Burdett points out, to the system adopted in America for regulating the admissions to the free beds. "Americans hold rightly that no person is entitled to occupy a free bed unless or until he can prove beyond dispute that he is unable to pay something for the treatment he receives in the hospital ward. It is not the business of the managers to exert themselves to ascertain the circumstances of each patient, because every applicant for relief is bound to present his own credentials, clearly defining how far and to what to extent, if at all, he is able to pay for the relief he receives. . . . The reason why the English system has steadily deteriorated is that the managers have proceeded on a wrong principle when dealing with the circumstances of each patient. It has become the practice to consider it to be a part of the duty of the managers to enquire into the circumstances of the patient, instead of insisting upon each patient providing adequate credentials as to his circumstances and means. The English practice, is, in fact, the exact converse of that in force in America,; and to this cause initially, and to this alone, most, if not all, the abuses connected with the English system are due."

At the Meeting of the British Medical Association, at Sheffield, in 1907, this problem of how best to deal with the abuse of hospitals was very fully discussed, and a resolution was carried, "that some means of investigation into the circumstances of the applicants for relief, by means of an almoner, or other agent, shall be employed in all medical charities."

This method is being very extensively tested both in England and elsewhere at the present moment. It perpetuates the old English method of dealing with this abuse, which hitherto has not been successful, and I see very little reason to hope that it will be more successful in the future. This same question of hospital abuse, and the same problem of how best to deal with it, formed the subject of a long and interesting discussion at the Australasian Medical Congress held at Melbourne in 1908. This discussion is of particular interest to us, inasmuch as the conditions in Australia resemble much more closely the conditions in this country than do those of Great Britain or of the other European States. Among the resolutions adopted by that Congress are three which specially deserve our attention. First: "That in the opinion of this Congress no payment whatever should be taken from patients treated at hospitals maintained by the donations of the charitable and by Government grants." Secondly: "That, except in cases of accident or emergency, every patient should, before being treated, be required to sign a declaration of inability to pay for medical attendance; and that, as far as possible, all patients seeking treatment should bring a recommendation from their own medical attendant, whether lodge or other." Thirdly: "That this Congress affirms the foregoing resolutions as the foundation of any real reform in the present abuse of hospitals, and is of opinion that other hospital accommodation would extend naturally to the inclusion of all those able to pay for medical treatment."

To put it shortly, the Australasian scheme for doing away with hospital abuse, or controlling it, is to have three kinds of hospitals. First, hospitals supported by subscriptions and government grants, which should charge no fees whatever to the patients and which should admit none but the absolutely poor. Secondly, intermediate hospitals, which should admit part-paying patients. It is not laid down how these hospitals should be financed, whether they should be Government institutions, or an extension of the work of Friendly Societies, or branches of Life or Accident Insurance Companies, or Co-operative efforts, or private ventures. But, however organised, the payment of the medical attendant is part of the scheme; for it is held, and on general principles I think rightly held, that too much gratuitous work is expected and exacted from members of our profession. It is not fair to the profession, and it has a distinctly demoralising effect upon the public. In his address in Medicine at the Sheffield meeting of the British Medical Association, 1907, Dr. J. K. Fowler said: "But how will the practitioners fare for a livelihood in the future, if the ever-increasing tendency of the people to expect gratuitous medical attendance continues unchecked, and neither political party considers it its duty to encourage habits of thrift amongst the working population?" It may be objected to by those outside the ranks of our profession that in all this we are looking after our own interests; that the motive power behind us in this movement is the selfish one of trying to secure the loaves and the fishes. Let me at once acknowledge that it is our duty to care for the interests, material as well as social and moral, of our profession; that we are right to look after ourselves; but let me also say that this is not the only nor, by a long way, our chiefest object. It is for the good of the whole community that the profession should be protected, and its interests safeguarded; that its position should be unassailed, or even improved; that its members should receive a just reward for the work they do, so that the profession should attract the best material to its ranks, and not the refuse from other professions, nor the failures in other walks in life. The medical man, let me repeat, has always been noted everywhere for his sympathy with the sick and his generosity to the poor; but, as Dr Davies, President of the Transvaal Medical Council, said, he likes to do his charity himself, and does not want outside assistance. He is quite willing to give his services gratuitously to the sick poor, but he objects to this hospital abuse, whereby it is sought to commandeer his services, without fee or reward, for those who are well able to pay but who by misrepresentation and fraud take advantage of our hospitals. This same abuse affects the Johannesburg Hospital. Previous to the war there were, comparatively speaking, few free patients; nearly all applicants were content to pay the hospital charges, and many of them their medical fees as well. But since the war a change has come over the scene. Whole wards which were pay-wards are now free, and hospital abuse has enormously increased. The increase in the number of free beds is, no doubt, due to the period of profound depression through which the country has passed. There can be no question about this: that the working classes especially, who used to pay without a murmur for hospital treatment, cannot always do so to-day; and, *pari passu*, with the increase of the number of free beds there has grown up a system of hospital abuse. For several years relief on a large scale was organised, and in spite of all the precautions which were adopted by the organisers, the deserving and the undeserving alike benefited by it. I am inclined to think that this had no small share in the demoralisation of the people and in the increase of hospital abuse. Thus in the Transvaal we have the same problem before us as you have here, as they have in Australasia, and as they have in Great Britain. How is it to be remedied? The late Colonial Secretary, through his mouthpiece, the present Secretary of the Interior, promulgated a scheme, which found expression in a letter dated November 29th, 1909. I believe that letter was very widely read by those interested in these matters, so it is not necessary that I should repeat it. But before criticising it, I should like to say this:—Technically the Colonial Secretary is responsible for everything which emanates from his office, but I cannot believe that he is more than technically responsible for this scheme. I believe that he is most anxious personally to put all matters right which affect the medical profession and the public health of this country; and, moreover, I believe he is far too able and too astute a Minister to have promulgated this scheme if he had had the time and taken the trouble to sift its principles and to understand its bearings. It would seem that the brilliant idea contained in this scheme is to try and minimise hospital abuse, so far as it affects hospital funds, by legalising that part of it which affects the profession. That is, it proposes to compel the honorary staff to treat gratuitously all and every patient in the hospital, regardless of his circumstances, while taking steps to exact, in whole or in part, the hospital charges. I am inclined to think that the writer of that letter was very well pleased with his composition, but its contents and its tone were resented by the whole honorary staff. The concluding sentence of their reply reads: "In brief, the honorary staff are always willing to help those who cannot afford to pay for their services, but refuse to be exploited, through the hospital, for the benefit of the well-to-do, which is practically what is proposed." The Colonial Secretary's Department may not have been impressed by the views of the honorary staff of the Johannesburg Hospital, since in a subsequent communication it writes: "The private interests of the medical profession, even though the efficiency and well-being of the members of that profession are in no small degree a matter of public concern, must come as a secondary consideration in the conduct of a public hospital." Perhaps that Department would have been more impressed if it had seen and read the comments on its letter made by a most influential and unbiassed outside authority. A leading article which appeared in the *British Medical Journal,* on March 19th last, referring to this same letter, which was published "by authority" in the *Transvaal Leader,* says: "It was originally addressed to the Chairman of the Committee of the Hospital, and gives that Body instructions to the effect that it must reduce its charges to paying patients, and discourage such paying patients from exercising their right of being treated by their own medical attendants. The avowed object is to make medical treatment in hospital so cheap that the application for admission to free beds may be reduced to a minimum. In other words, the object is to make the hospital a financial success at the expense of the medical profession. Apart from the coolness of this proposition itself, the whole tone of the letter is one to which the medical profession has a right to take exception. No public authority is justified in writing as if the views of medical men concerned were of no importance."

Now, it has been the practice in the Johannesburg Hospital for the honorary staff not only to treat gratuitously all free patients, but also to charge no fees to any patient who was not in a position to pay the full hospital charges. Moreover, if a physician or surgeon in charge of a case had reason to believe that a patient's means, while able to meet the full hospital charges, were not sufficient to pay medical fees as well, no medical fees were charged. I take the Johannesburg Hospital, because it is the one with which I am best acquainted, and also because it is the largest in South Africa. In 1907 the total number of beds was 396, and when the new wards are completed the number of beds will be 500. We have seen that in Australia, in order to combat hospital abuse, it is proposed to admit none but free cases to their general hospitals, because experience has shown that where free patients and part-paying patients are mixed together, it is impossible to prevent fraud and abuse. They also propose that in their so-called intermediate hospitals the payment of the staff shall be an integral part of every scheme. This is a very difficult question, and there is much to be said on both sides; but in the Johannesburg Hospital I don't think any member of the staff has any wish to depart from the practice which they have hitherto carried out in regard to part-paying patients; while at the same time we are most anxious to check the abuse which exists, and which has steadily been becoming more flagrant. I do not think these two are incompatible, though they may be when the free and the part-paying patients are mixed up together in the same ward, as is proposed in the scheme which emanated from the theorists in the Colonial Secretary's Office. It is devoutly to be hoped that the time will never come in Johannesburg when 500 free beds will be required. I think the best way of dealing with this question of abuse would be to keep the free and the part-paying patients absolutely distinct. I cannot see the force of the argument, that by putting them together in the same ward, and treating them exactly alike, more applicants for admission will be inclined to come in as part-paying patients. The converse is far more likely. Have free wards, and strictly limit the number of free beds, and let far more care be taken to allow none to enter the free wards, except the absolutely poor. There are many who have still sufficient self-respect to do what they can to pay something, rather than have it said that they were treated for charity in a free ward. But for those who have no such scruples, who rather think it a clever thing to mislead the authorities and "do" the hospital, we must find some means of dealing with them. An almoner has been appointed whose duty it is to enquire into the circumstances of every applicant. That is a move in the right direction, but it is not original. It is the custom, practically, in every large charitable institution of the kind in Great Britain, and it has proved only a partial success. The American plan of requiring every applicant to produce evidence of his eligibility, to prove his poverty, has proved more efficacious. A letter of recommendation from a subscriber, from a magistrate, or from a clergyman is useless. A letter from an applicant's regular medical attendant would be the best safeguard. The same would apply to the part-paying wards, where all would pay something, but by this method they could be graded according to their means. The Hospital then would not be abused, and the working classes would be encouraged to exercise thrift. In the Colonial Secretary's letter, he says: "Under the conditions which obtain in South Africa, a public hospital has to provide not alone for indigent poor, that is people who in health are poverty stricken and wellnigh destitute, but chiefly for a very large class of persons who in health earn a fair and even a comfortable living, but who by failure to make any provision against sickness find great difficulty in paying for their treatment when sickness or injury overtakes them." This, of course, is quite true; but how does the Colonial Secretary's Department propose to deal with it? By putting a premium upon thriftlessness! By putting the thrifty man, who out of his savings can contribute something towards the care and treatment he requires, into the same ward, in the next bed to the thriftless man who has made no provision during his prosperity, and is now depending entirely upon the charity of the hospital!

Gentlemen, there is one other point connected with hospital management to which I would like to refer, because in South Africa it is hopelessly misunderstood and the result is disastrous; but before dealing with it, I want to take this opportunity to correct a mistake, which is very commonly made, even by laymen who have to do with the management of hospitals. It is not necessary to say it to you who are gathered here, but as I hope this discussion, which is being now inaugurated, will be read by people who do not belong to our profession, I think it is only right that I should refer to it. I have over and over again referred to the medical staff of a hospital as being composed of men who *give* their services to the poor and the indigent without fee or reward. It is very commonly supposed that the physicians and surgeons to a hospital are in some way or other paid for their services. I have known a member of a hospital committee not only say that the members of the medical staff were amply rewarded, but to hint pretty broadly that they took advantage of their position to exact fees from free patients. And when he was corrected he received the correction with an incredulous and insulting smile. These people say, if there is nothing to be gained from a hospital appointment, why should there be such anxiety on the part of medical men to get these appointments? Let me explain the matter once for all. It is quite true that in Great Britain and Ireland, as well as in the other countries of Europe, there is a distinct and a very real advantage in being attached to one of the large hospitals, especially those which have medical schools attached to them. In the latter a hospital appointment carries with it, generally, a Professorship or Lectureship in the school, the emoluments of which are frequently of considerable value. But even in those which are not directly connected with a medical school, they are nearly all attended by medical students who are obliged to take out hospital courses, and have to pay for the instruction they receive. It is not necessary for me, in this connection, to explain what an advantage this is to the hospital itself and the patients too. These hospital fees naturally go to remunerate the members of the staff, who give the instruction. The better the instruction given, the more students are attracted to the hospital, and the remuneration of the staff benefits in consequence. In this way a hospital appointment is directly of benefit to the holder. Indirectly, it is perhaps of greater value still, or it may be the reverse. For the presence of students ensures publicity. They talk. They dissect minutely every member of the staff. His character, his temper, his knowledge and his skill are freely discussed. Every success, and every failure or mistake, is noised abroad. Through the student every man connected with a hospital is known. Through them his reputation is made, or marred. The nursing staff, especially in a hospital which trains and supplies nurses to the outside public and not only for use in the hospital itself, is another useful channel for advertisement. Hence a hospital appointment in the Old Country is a very valuable asset to a young man starting in his profession. But in this country it is a very different thing. There are no medical schools, and there are no medical students. The best work, the most skilful diagnosis, the most brilliant operation, as well as the greatest mistakes, the most glaring failures, may pass absolutely unnoticed. I say a hospital in this country, not even one so large and so well equipped as the Johannesburg Hospital, adds practically nothing and detracts nothing from a man's reputation. It certainly gives him a larger experience and in that way is of value, if he chooses to take advantage of it. But if he be a man with a large outside practice it will absorb so much of his time as almost to neutralise its other advantages. To say or to suggest that he draws any pecuniary advantage from it is absolutely false. It is an honourable understanding, which I believe is invariably acted upon, that no member of the staff takes fees from any patient unless the patient is in a position to pay the full hospital charges, and for a member of the lay committee to suggest the contrary is not only an insult to an honourable profession but shows an ignorance for which there is no excuse. And that takes me to the last point to which time will permit me to refer. It is one of very great importance to the well-being and to the smooth and harmonious administration of our hospitals, namely, the relations of the managing committee to the medical staff. There are three systems in vogue in the hospitals at home. In some hospitals all the members of the honorary visiting staff are, *ipso facto,* members of the committee; in some the medical staff elect two, three, or more representatives on the managing committee. As a variant of this second system, the members of the visiting staff do not sit on the committee, but when a physician or surgeon retires from the visiting staff and becomes a consulting physician or surgeon, he then takes his seat on the committee. And lastly, in a few hospitals the medical staff is not represented on the committee of management. In all, however, it must be remembered, the honorary visiting staff constitute a Medical Board. In all, this Board has well defined powers and privileges, and nothing is done by the managing committee in regard to the medical department without consulting the Medical Board and obtaining its approval. The *British Medical Journal,* in an article in its issue of March 19th last on "The Medical Profession in South Africa," says: "In this country (that is, in England) in particular an increasing degree of influence in the control of hospitals is being yearly accorded to members of the medical profession, and, as is admitted on all hands, with great advantage." In South Africa, however, as regards the management and control of hospitals, the medical staff is practically of no account. The relations which exist between it and the Managing Committee are strained and chaotic. This state of affairs, which is not only deplorable but is inimical to the best interests of the hospital itself, exists all over the country but is found in its most acute form, perhaps, in the Frere Hospital at East London and in the Johannesburg Hospital. One cause for this I have alluded to already, namely, that when the hospitals were first started there was not sufficient cohesion, sufficient *esprit-de-corps,* among the medical men themselves, to enable them to assert their claim to a voice in the management of the hospital. And this is the more to be regretted because they were practically the only men in this country who had any experience or knowledge as to the lines upon which a hospital should be conducted. Most of the hospitals at home have long histories and the newer ones have had the benefit of the experience of the older ones to guide them. The members of the managing committees in these hospitals are gentlemen chosen by a large body of governors, chosen on account of their experience and knowledge, frequently on account of their generous contributions to the hospital. They have learnt by experience that there is no class in the community that takes a keener or a more unselfish interest in the efficiency and good government of a hospital than the honorary visiting staff. They are not always looking for sinister motives in every suggestion made by the visiting staff; they recognise that in certain matters affecting the medical department the medical men must know best, and consequently mutual respect, the most friendly relations and the completest harmony prevail. How different is it in this country! Let me instance the Frere Hospital, where for a long time the relations between the Managing Committee and the medical staff, which I believed consisted of four, have been strained. Some time ago the Committee resolved that the Minute Book of the Medical Board should be submitted to them at their meetings. I have never heard of such a demand being made before. The Medical Board refused, and I consider were perfectly right in doing so. Subsequently the Committee appointed all the medical men in East London as members of the staff, some twelve to fourteen in number; and the new appointments outvoted the old. Comment is unnecessary. I have elsewhere called attention to the state of affairs in the Johannesburg Hospital where a Commission, consisting mainly of members of the Managing Committee, was appointed to report, *inter alia,* on the management of the hospital. The Chairman of the Commission was a member of the Committee. I believe he and other members of the Commission were most anxious to promote harmony between the Medical Board and the Managing Committee. In my evidence I gave my experience of the Adelaide Hospital in Dublin, where I was one of the representatives of the Medical Board on the Committee for a period of over twelve years, and where the pleasantest relations existed. I strongly urged that to secure harmonious working in the Johannesburg Hospital the first and most important consideration was that the medical staff should be represented on the committee, with full voting powers. I believe that every medical man who gave evidence before that Commission bore similar testimony. But in spite of that evidence the Commission reported that the medical staff should not be represented on that committee, nor on any committee in future in any capacity. Gentlemen, could anything be more crassly silly, or more obtusely ignorant? How is it possible in the face of such a resolution to secure harmony? How is it possible that a well-meaning but inexperienced committee can bring order out of chaos, when they deliberately shut their ears to the only expert advice available? It has over and over again been pointed out that the medical staff do not want the power to outvote the committee—they ask only for adequate representation, and by "adequate" they have explained that all they want is that no hospital committee shall act blindly and without knowledge; that they shall have two or three representatives on the committee who shall be able to explain what their views are, what experience has taught, so that if the committee go astray, they shall not be in a position to plead ignorance.

And so we have asked that in a United South Africa there shall be an adequate representation of medical men on all committees or Boards which have anything to do with the hospitals. Surely that is not asking too much. We are not infallible, we do not claim to be; but our experience and technical knowledge insure that we are not so fallible as lay members of a committee, however well-meaning, who lack our experience and knowledge, and are not aware of it.

Discussion.

Dr. Darley-Hartley said he had listened to the very exhaustive paper read by Sir Kendal Franks with much pleasure. He himself was chairman of a committee appointed some time ago by the Western Province Branch of the British Medical Association to go into the question under consideration. That committee sat for a considerable time, and they endeavoured to get all the information possible, not only in Cape Town, but from the hospitals throughout South Africa. The general conclusion they came to on the whole—certainly it was his own—was that the best method of reform was on the lines suggested by Sir Kendal Franks, as having been adopted by their

Australian brethren, and his remarks deserve great weight. The essential feature of the Australasian conclusions he had very much sympathy with, and this increased the more he studied the subject. If they wanted to prevent the abuse of the hospitals and defend at the same time the medical profession, the great thing was to have a separation between hospitals for free patients, and hospitals for partial or full-paying patients. Sir Kendal Franks had not touched upon one very great reason for that, but there was this feeling constantly present in the minds of a certain class of patients, namely, if a patient went to a hospital which was partly a non-paying one, that remnant of self-respect which made a few people even nowadays resent the stigma of pauperism disappeared. The patient had been, for instance, to a particular hospital—say at Johannesburg—where free patients were admitted, and where also patients went who paid a guinea a day. Well, he went there and talked about having been treated at the Johannesburg Hospital, but he did not tell anybody that he was there as a free patient—he led people to infer that he paid a guinea a day—as those who paid were on the same level as those who were free. Then, again, there was the intermediate class of patient, who paid perhaps 2s. 6d. or 1s. or even 6d. a day, and they might rely upon it that these people went away with the comfortable feeling that they had paid their way, however small the amount contributed: they did not realise that they had been the recipients of any charity for which they had to thank anyone. Doubtless they thought the expense was borne by the Government or Town Council or somebody else. He therefore urged that the two classes of patient should be separated. Then, again, it was found that oftentimes friction took place in wards where there were free and full paying or partial-paying patients. The free patients were invariably more exacting and wanted more done for them than those who paid. Sir Kendal Franks alluded to the Socialistic idea that every member of the community, whether poor or paying taxes, was entitled to the benefits of hospital treatment, but it was essential to counteract that idea. It was the thin end of the wedge, and it was pretty general in Johannesburg and also in Natal and Rhodesia, this idea that, *primâ facie*, hospitals were for all members of the community. What did this mean? If it went on, it meant nothing less than the socialization of medicine. Medicine was going to be practised in State-supported institutions, and there would be employed in the hospitals one, two, or three whole-time salaried medical men, doing the same sort of function as the Superintendents of the great Poor Law and Infectious Diseases Infirmaries in England did, and these medical men, who were not allowed private practices, would be utterly over-worked. All the surgery and medicine of any account would go to these hospitals, and they would have two classes—one of skilled physicians and surgeons over-worked inside the hospitals, and outside, medical practitioners attending to minor ailments, having about the same professional status as the apothecary 150 years ago; in fact, the better class of medical practitioner would become a vanishing quantity. Hence the first thing they must do was, in the interests of the public, to stop the idea that a man was entitled to medical attendance in hospital because he was a member of the community. The best method to adopt was to insist upon a separation of the two classes of patients as far as possible. Let the State provide free hospitals for the poor, those even who while earning fair wages in time of health could not lay by for illness, and let it cease giving any aid to a hospital unless *pro rata* to a certain number of free patients.

Dr. Casalis said he had listened with great interest to the paper read by Sir Kendal Franks, and he quite agreed with him that a hospital appointment to a medical man did not mean increased emolument; on the contrary, it was frequently a loss. He was himself at one time connected with one of the hospitals here, and so far as his private practice went it was anything but advantageous from a financial point of view. Nor was he the only one. Dr. Sharpe did a large amount of work at the Woodstock Hospital—mostly free cases—and although he gained experience, financially he was a loser.

Dr. Rogers said he disagreed with what Sir Kendal Franks and the last speaker had said on this subject. He thought they could not get away from the fact that in their own interests it was an advantage to be attached to a hospital staff, apart from monetary considerations altogether. Those medical men who were attached to a hospital staff were greatly benefited, and their experience went on increasing all the time; this was why so many medical men endeavoured to get on a hospital staff. In a discussion they had with the Colonial Secretary in the Transvaal on hospital administration they endeavoured to rub this in, the value to the profession of hospital appointments, and eventually they were most valuable, as medical men gained experience. If Dr. So-and-so was known to have got his experience on a hospital staff, his practice, and consequently his fees, improved. With reference to the question of managing committees, his own opinion was that the Board should consist entirely of medical men who had technical experience, but he doubted if that could be carried. In South Africa they must face the certainty of having a lay Board, and the authorities themselves supported that view. They met General Smuts on this particular question, and previously they met Dr. Jameson, who also upheld the principle of lay Boards, but at any rate appointments to vacancies on the staff should be made by the staff, and in selection the whole outside profession should be given representation.

Dr. Ireland said, with reference to the constitution of the Board, about eleven years ago, when they started their hospital in Cradock, they endeavoured to have two representatives of the staff placed on the Board; it had been found very advantageous in every respect and worked admirably. Their staff was elected by the Board of Management and there had never been any friction. He had been himself placed in the proud position not only of senior visiting officer, but also of Chairman of the Board of Management.

Discussion adjourned until Thursday, November 3rd.

On resumption, Dr. Ward in the chair.

The Chairman: I would suggest that at this meeting we try to adopt some sort of scheme to which we are all agreed, and then the matter can be thoroughly thrashed out at the business meeting to-morrow. We might formulate some resolutions this afternoon, so as to forward the business.

Dr. Darley-Hartley: I thing that would be the best plan.

THE CHAIRMAN: I know Dr. Matthew Hewat has been working at this matter and, with your consent, I think we might call on him to resume the discussion on the subject.

DR. MATTHEW HEWAT: I have drawn up some resolutions, as I thought that, instead of a discursive discussion, we could arrive at something concrete, and with that view I proposed to group our necessities in three different sections. In the first place, as a profession, we are always ready to attend paupers, or those unable to pay for medical assistance, gratuitously. Acting upon that, I thought we might pass a resolution to the effect that hospitals for the accommodation and treatment of paupers or those unable to pay should be provided by the State, and that such pauper hospitals should be at all times separate from any other description of hospital. There should be no mixing up whatever of a pauper hospital with anything else. That is the first proposition I want to bring forward, namely, that whereas the medical profession is now and has always been prepared to attend paupers gratuitously, the Congress affirms the principle of establishing pauper hospitals, State-aided, throughout the country, such pauper hospitals at all times to be separate from hospitals receiving payment for nursing.

The next point I wanted to bring forward is, so to speak, non-controversial—that in the case of individuals capable of paying for medical treatment or surgical skill in nursing homes, where such homes exist, no hospital should be started by the Government, but patients should be compelled to go to private nursing homes and not be provided for out of State funds.

DR. DARLEY-HARTLEY: You mean patients able to pay?

DR. MATTHEW HEWAT: People able to pay full fees for nursing homes where such homes exist. There, no State-aided hospital should be started. In a big hospital, the paying department should not be mixed up with other wards, and such full-paying patients would be entitled to medical and surgical attendance by their own medical men.

DR. DARLEY-HARTLEY: Where nursing homes do not exist, if there is a separate department in the hospital, it should be kept financially separate from the hospital generally.

DR. MATTHEW HEWAT: If possible. We have to face this position. In this country there may be a number of little villages where they cannot support a nursing home, but they have cottage hospitals. That, however, is quite a different thing from what we are now discussing.

DR. BROWN: I think, if possible, it would be better to avoid the expression "pauper hospitals." If you said hospitals which do not receive paying patients it would sound better.

DR. MATTHEW HEWAT: I would beg to move the following resolution:—

> "Whereas the medical profession is now, as ever, prepared to give free assistance to those unable to pay for same, this Congress affirms the advisability of the establishment of hospitals entirely or partly supported by Government for such cases only, and in centres large enough to warrant such establishment.

These establishments should be entirely apart from any hospital or department of same receiving payment in part or in whole from its patients."

Dr. Darley-Hartley seconded.

The Chairman: I should support the resolution, and am entirely in accord with it, but at the same time I consider that the State should, in the ultimate resort, be entirely responsible for the up-keep of the hospitals, because you do not find here much in the way of charitable subscriptions or donations to these institutions. In Bloemfontein, an attempt was made by the Hospital Board to obtain subscriptions, but with very little definite or tangible result. The consequence has been that the Government has been called on, through force of circumstances, to take upon itself the full financial responsibility of the National Hospital at Bloemfontein. I understand from Dr. Hewat that this resolution refers mainly to the large centres, and I quite agree with him. I think in large centres such hospitals as are outlined in the resolution should be kept absolutely and entirely distinct, that is, as regards non-paying and semi-paying or full-paying patients. This, I feel sure, would tend largely to prevent hospital abuse which is so rife, and obviate that feeling that often arises where paying and non-paying patients are lying side by side in the same ward. It would also tend to better supervision.

Dr. Balfe: I would like to know if the proposal affects existing hospitals, or is it suggested to ask the Government to establish new ones on these principles?

Dr. Matthew Hewat: It works both ways. If an existing hospital is being run on definite regulations and nobody takes the trouble to alter things, it will go on as it is until the advantage of adopting right methods is seen.

Dr. Darley-Hartley: I would like to say that General Scobell, when Acting Administrator of the Cape Colony, in an address he gave at the annual meeting of the New Somerset Hospital, laid down the principle that in his opinion the State, and the State alone, should take over the whole task of providing for the people who cannot pay, and that there should be intermediate hospitals for those who can partly pay. I think the principle of the resolution is a good one. The Government could make a grant in proportion to the number of free patients.

Dr. Ireland: I might mention that I am one of the members of the staff of the Cradock Hospital. We are a State-aided hospital, receiving a grant of nearly £1,000 a year, which, of course, goes a long way in maintaining the institution. Besides that we have paying patients. Those who contribute something towards their maintenance and treatment, above 5s., are put in the general ward with the ordinary patients, but they are supposed to pay the medical fees. Then there are private wards where the charge is 10s. 6d. a day, so that gives us an opportunity of treating cases of a surgical nature. The secretary sees that the privileges of the hospital in regard to free patients are not abused, and is very vigilant, so we have always been able to pay our own way and keep free from debt.

Dr. Elliott said this was an extremely large question, and he was inclined to agree with Dr. Hewat's resolution in the abstract, if they took it for granted that the hospitals were supported only by the State. In England, the ordinary hospitals were supported very largely by charitable contributions, and, in a sense, this was the glory of English hospitals.

Dr. Matthew Hewat said what they wanted was to get rid of the anomaly of paying and non-paying patients being accommodated together. Either patients were paupers or they were not, and they must get this rubbed into the mind of the general public as soon as possible.

Dr. Ireland said the public and railway men paid 3s. 6d. a day, and they had refused to pay more. Would they be treated as paupers, or what provision would be made for them?

Dr. Matthew Hewat: That is a somewhat difficult matter to deal with, and the same with regard to clubs. I do not see very well how we can legislate in regard to the Government Sick Funds.

Sir Kendal Franks: The Johannesburg Hospital now has 396 beds, and it will shortly have enough free patients to fill 500 beds. I entirely agree with the resolution in regard to all other hospitals for the future, but what we will have to do in the Johannesburg Hospital is to put aside a certain number of pavilions for the absolutely free cases: nobody would be admitted to these except paupers —the poor and destitute,—and the other pavilions would be used only for part-paying patients. At present the hospital charge is 12s. 6d. a day. Any patients who cannot afford to pay 12s. 6d. a day the staff treats free, and the difficulty is to find out and be sure that they cannot pay more. The part-paying patients are treated free by members of the staff, but I think we ought to abolish the system of taking in paying patients. Those patients who can pay full fees should be excluded entirely.

Dr. Matthew Hewat: You think that a pauper patient should be under a separate department of the hospital?

Sir Kendal Franks: Absolutely.

Dr. Elliott: You do not exclude the idea of a second department?

Dr. Matthew Hewat: We cannot.

Dr. Reynolds: How about the Sick Fund people?

Dr. Hewat: I do not think we can interfere in that. The Government might turn round and decline any State aid.

Dr. Darley-Hartley: At East London the railway patients pay 3s. 6d. a day, but the Railway Medical Officer attends them in hospital. He is appointed nominally on the staff to enable him to do so, but he does nothing else in connection with the rest of the hospital. The patients in question are classed in that hospital as paying patients, but the ordinary hospital staff does not attend them.

Dr. Ward: At Bloemfontein the railway patients come into the hospital at 5s. 6d. a day and the Railway Medical Officer attends them.

Dr. Elliott: I doubt whether that will not confuse the two classes of hospitals which we are trying to separate. If the Railway Medical Officer follows cases into the hospital, we then have a sort of intermediate hospital again, and that is what we want to avoid. If we put these patients into the intermediate class, then they are not exactly paupers. I may say that we have done our best to prevent private beds in the New Somerset Hospital; the railway people ought to go there and not into the pauper hospital.

Dr. Fuller: Do I take it that your resolution means any hospital which is accepting both pauper and semi-paying patients?

Dr. Matthew Hewat: We would like separate hospitals, but I am afraid that cannot be done. We say we will deal with paupers and provide for them gratuitously, but they should be entirely apart from anything else.

Dr. Fuller: I think the expense of administration would be enormously increased. I think we ought to stick to the principle as laid down that Government institutions should be pauper hospitals

Dr. Reynolds: We had an enquiry not long ago into this very question, and one suggestion was to provide wards in such a way that one part of the hospital would be paying and the other non-paying, but that did not seem to work. Then we suggested putting cards over the beds of those not paying, but that would not do. There was another suggestion to put up screens. We are of opinion that we should separate the paying and non-paying and semi-paying patients, and I take it that the resolution emphasizes the necessity for doing that. As to the mode of carrying out the resolution, that will be for the Government or the hospital managers to see to.

The resolution was then put and carried unanimously.

Dr. Matthew Hewat: I would then propose the following:—

> "Where satisfactory nursing homes exist which are available to any registered practitioner, patients able to pay for treatment at such homes shall not be admitted to State-aided hospitals."

Dr. Elliott seconded.

Dr. Caporn asked: What about the amount of fees to be charged for admission to nursing homes?

Dr. Ward: That varies very much in different localities.

Dr. Brown: What will you do with the Johannesburg Hospital? It will shut out all those paying full fees there. Sir Kendal Franks seemed to think the Johannesburg Hospital would never be filled by pauper patients.

Dr. Darley-Hartley: Johannesburg may increase to double or treble its present size, and it may not continue to be so wealthy as it has been in the past.

Sir Kendal Franks: Without attempting to prophesy as to the future of Johannesburg, I think the hospital will always be full of free and part-paying patients. I think we ought to do everything to encourage the formation of private nursing homes for patients who can well afford to pay. In Johannesburg several have been started, some have been successful and some have failed, and they failed because they could not compete with the paying wards in the Johannesburg Hospital, where the patients are admitted at a rate which would not keep a private hospital going, and where they were able to pay full fees to medical men. I would rather see it laid down as a principle that these full-paying patients shall not be admitted at all.

The resolution was carried unanimously.

DR. MATTHEW HEWAT said the next point, and it was a difficult one, was the question of dealing with the sick poor, those between the classes dealt with in Resolutions 1 and 2. Every hospital ran its patients at so much a head per day, say 5s. He would propose the following resolution:—

> "That to meet the necessities of patients in large centres only able to pay part fees for hospital accommodation, either intermediate hospitals be established or separate departments of hospitals be set aside for such purposes."

SIR KENDAL FRANKS seconded.

DR. DARLEY-HARTLEY said that in Australia there were intermediate hospitals, and it was quite possible in large and moderately large centres to run intermediate hospitals for people of moderate means at an extremely low rate. In Switzerland such a system prevailed, and in some places the charge was as low as four francs a day. The medical men attended these moderate paying patients at a reduced fee, or in some few cases for nothing at all. There was also a hospital in London worked on much the same plan. A certain number of charitable persons agreed to guarantee the institution and find the preliminary capital, and a very low fee was charged to patients, the medical men accepting a certain scale of fees for cases and in some instances working for no fee at all.

He had discussed this matter with Dr. Sharpe of the Woodstock Hospital, and he thought it was quite possible to run an intermediate hospital in a centre like Cape Town on the ward principle that could pay its way at something like 6s. or 7s. a day. A good deal had been said as to full-paying patients, and he might remark that in Queenstown there was a private institution run by a lady which was most successful; but these institutions could only be established in fairly large centres. His opinion was that it was advisable in large centres that intermediate hospitals should be established, partly self-supporting and partly subsidized by private charity, and open to any medical practitioner. Where this could not be done there might be a special department of the cottage hospital. He should like to affirm the principle that if there was a cottage hospital the Government should contribute very liberally, but they should contribute nothing whatever to part-paying patients.

DR. ELLIOTT said that no doubt the conditions in South Africa had to a large extent brought about the present state of affairs owing to the floating nature of the population. In a place like Johannesburg, for instance, there were numbers of young men, not earning much perhaps, who were provided for by the general hospital. They paid a certain amount, and this had been the secret of hospital abuse to a large extent. He quite agreed that they should have intermediate hospitals, but he very much doubted whether it was a matter of practical politics to persuade a management to shut its doors to patients who paid a certain amount.

SIR KENDAL FRANKS: It would be impossible.

Dr. Elliott: That is what I fear. In Edinburgh an attempt has been made to form an intermediate hospital.

Sir Kendal Franks: I do not think it is well to fix any sum. I would leave that intermediate. In Australia, the principle is that the medical man should be paid a *pro rata* share out of whatever the patients pay, but that does not commend itself to my mind. I would say, instead of establishing intermediate hospitals, "intermediate departments of a hospital." We should try and get as many people as possible to pay something.

Dr. Elliott: How would you provide for the class who cannot pay full fees but who would like to pay a certain amount to their doctor—I mean people who cannot go to the expense of a nursing home? Ought they not to pay something for the doctor? In the New Somerset Hospital we attend them free while they pay 6s. 6d. a day to the hospital.

Dr. Matthew Hewat: If a man can pay 10s. a day, the doctor is entitled to 5s.

Dr. Darley-Hartley: I do not see why the medical man should give all to charity and the hospital only give a fraction. The public do not realize what the doctor does in the way of charity.

Sir Kendal Franks said he thought that everyone of these intermediate hospitals should be supported, or at all events subsidised, by the Government.

Dr. Reynolds said that in Natal he was member of a Commission to enquire into Cottage Hospitals, and they found that in sparsely populated districts the district surgeon was the medical officer of the cottage hospital, but in the case of any patient he wished to send into hospital he lost all his fees. They changed all that, and now, if the district surgeon or anyone else was attending a patient in the village, he sent the patient into the cottage hospital, and the Government charged what they made out to be the daily average cost of maintenance, and the doctor charged what fee he liked. At present the cost was about 7s. 6d. a day, and the fees between the doctor and his patient had nothing whatever to do with the Government. The Government only charged for maintenance and for nursing. In the case of patients not able to pay the full charges, then the district surgeon was asked to attend them free, but then the district surgeon received a small salary for attending the hospital and attending paupers. That was the system at present in vogue in cottage hospitals in Natal, and it worked very well.

Dr. Elliott: The difficulty of applying that to a large hospital is that the staff of a large hospital would decline to take fees in a lump like that: they do not desire to be in a different position to that of other medical practitioners. If a doctor is paid, then the cases should choose their own doctor. It is not the staff of a large hospital that should get payment preferably to other doctors.

Sir Kendal Franks said that in a country like this it was very difficult to get subscriptions to keep hospitals going, and it was most unfair that the burden of support should fall on a few people who were charitably disposed. People who had the most money would not contribute unless their names were published. Originally in Johannesburg a tax of a shilling a head per month was paid by employers for hospital purposes, and in the fourth year this tax brought in over £100,000.

Dr. Keegan said that this subject had been considered at Graaff-Reinet. There they attempted to get a tax levied, but it was objected to, and the community would not submit to any tax for the support of a hospital, so the proposal fell through. They should, in his opinion, keep the Government out of the administration of hospitals in the country as much as possible, though this could not very well be done in the case of the absolutely poor. There, of course, Government assistance was necessary. He was in favour of the establishment of intermediate hospitals, and they should be run by the local community themselves. The community should not be asked to support in any way by charitable contributions hospitals aided by the State. If necessary, a local tax could be imposed for the support of intermediate hospitals.

Dr. Caporn said he thought the Government should contribute to intermediate hospitals: it was unfair to ask the charitable public to contribute. He would like to ask Sir Kendal Franks whether the shilling tax at Johannesburg was paid by the employer, or whether it came out of a man's wages?

Sir Kendal Franks: It is paid by the employer.

Dr. Matthew Hewat: What we want is to separate the pauper hospitals and the part-paying or intermediate hospitals.

Dr. Keegan: That means that we must have two administrations, I take it, and the expenditure for the paying or part-paying patients and the free patients would have to be differentiated, which would be most difficult.

Sir Kendal Franks: The resolution only touches the principle; it does not touch the question you allude to at all.

The resolution was then put and carried.

Dr. Ireland then moved the following resolution:—

> "That where no nursing home is available, patients be admitted into State-aided hospitals at fees from 3s. 6d. upwards, who shall be considered private patients able to select their own medical attendant, and be responsible for the fees to such attendant."

Dr. Keegan seconded.

Sir Kendal Franks said he thought it inexpedient to mention any sum specifically.

Dr. Ireland: I mention 3s. 6d. as it is the sum which the police and railway men pay.

Sir Kendal Franks: I think in Johannesburg the charge is 5s.

Dr. Ireland: We have tried in Cradock to get it raised, but the Government would not do so. At Bloemfontein, I am told, it is 8s. 6d. As the administration of hospitals will now come under the Provincial Councils it will be a matter for each Province to determine.

Dr. Matthew Hewat thought that in small communities cottage hospitals should be established, providing, where possible, in separate departments, for three groups of patients, it being understood that where the patient was paying less than 3s. 6d. a day their medical attendance should be gratis. If a patient could pay the hospital 3s. 6d. a day, he ought to be able to pay the doctor something, say a lump sum of a guinea for perhaps a month's attendance.

Dr. Darley-Hartley said he was inclined to support Dr. Ireland. It practically embodied a very good principle, namely, that if a man could not pay 3s. 6d. a day as a minimum, he should neither pay the hospital nor the doctor anything. He thought it would be very demoralising to go about trying to get patients to pay a shilling a day or so for medical attendance. If a man did pay a shilling a day, he probably went about talking of what he had done, quite ignoring that he had been assisted by anyone. If the doctor got nothing, then the hospital should get nothing.

Sir Kendal Franks: I do not agree with the last speaker at all. If a man goes into the hospital and can only afford to pay 2s., he should be allowed to do it. It is only encouraging pauperism to say that if a man wants to pay 2s. he is to be admitited free, as a pauper. They should be encouraged to pay whatever they can for the treatment they get.

Dr. Ireland's resolution was carried.

Dr. Darley-Hartley thought that in determining the class of patients under the foregoing resolutions the most satisfactory evidence was to be found in a certificate from the medical practitioner. When a question arose as to whether a patient should be free or should pay, say, 3s. 6d. a day, the medical man was the most competent to decide. This was the principle adopted in Australia and also in Natal. The medical man was the best able to judge of a man's position and pecuniary means. If he thought he could pay he was not likely to recommend him as a non-paying patient, whereas recommendations by clergymen, subscribers and others were often given recklessly. He had known cases where recommendations were given by employers to save their own pockets, and all that sort of thing. He begged to move:—

> "That in the classification of patients, the most satisfactory form of evidence is a certificate from a medical practitioner.

Sir Kendal Franks seconded.

Dr. Matthew Hewat said he agreed with the resolution. They must get the public to realize that hospital charity was partly medical charity.

The resolution was put and carried.

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